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SURGICAL TREATMENT OF TUBERCULOSIS OF THE KIDNEY[1]

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IT is my purpose to lay stress upon the incalculable value of radical surgery in the treatment of tuberculosis of the kidney, and to demonstrate that the recognition of this fact by the profession at large will greatly improve our statistics of renal tuberculosis. I feel strongly inclined, by way of an introduction, to make an excursion into the literature of this interesting subject, to show how surgery first gained its foothold in this new field, and how it has advanced by slow steps since then; and this inclination is the stronger because the progressive movement has taken place almost entirely since I treated the first patient of the series upon which most of my opinions are based. However, as time is short, I proceed at once to the question of surgical treatment.

My experience with tuberculosis of the kidney, excluding patients seen in consultation with other surgeons merely for diagnosis and several patients still under observation, covers 57 nephrectomies, 4 nephrotomies, and 1 partial nephrectomy. These operations have been performed either in the gynecological department of the Johns Hopkins Hospital or in my private hospital. The first patient of this series came into my hands in 1889, and the last is now convalescing.

Before we discuss the question of operative surgery, let us briefly consider the value of an expectant or medicinal treatment by noting the course of the disease in cases handled in this manner under careful observation. If we first clear the field in this way, we shall then be better able to gauge the significance of the surgical treatment.

Personally, I have yet to see a spontaneous or medical cure of the disease where the tuberculosis has been clearly demonstrated, though I have seen many futile efforts in this direction. Some of my earlier patients underwent the general fresh-air and forced-feeding treatment, so useful in pulmonary phthisis, and I had the opportunity of watching them month by month as they diligently pursued the measures advised by my medical colleagues, but not only did I never see a regression of the disease, but in nearly every case there has been a demonstrable advance. Added to this, a study of the specimens obtained by my nephrectomies does not afford a single instance of any kind of a healing process which preserves for the affected part of the kidney its functional capacity. In cases where the kidney has been completely destroyed and its ureter filled with caseous material, or occluded, of which I have had a number, there has always been extensive bladder tuberculosis, and in some instances tuberculosis of the second kidney as well. It is true, a sequestration or encapsulation of the diseased kidney, associated with an occlusion of the ureter may lead to a pseudo-cure, and many such cases are recorded. Experience shows, however, that even in these the disease may long remain dormant, ready at any time to light up afresh.

In questioning nature's powers to heal unaided, also, we must take into consideration the occasional extreme chronicity of the disease.

[1] Read at the nineteenth annual session of the Southern Surgical and Gynecological Association, Baltimore, Maryland, December 11, 1906.

In my own group of 62 patients, the average duration of symptoms, before consulting me, was as much as 3½ years. In one, the symptoms had been present for thirteen years, and there was some secreting tissue left in the diseased kidney, while its fellow was healthy. This is an extreme example. Again, *per contra*, a single year may see the onset of the disease in one kidney, its utter destruction, and the involvement of the bladder and the second kidney. With the rare exception of the sequestrated kidneys, the disease sooner or later involves the bladder. Out of my sixty-two, forty had more or less extensive bladder tuberculosis. In some, the bladder involvement takes place very early, while in others it is delayed for several years. I need not dwell upon the sufferings caused by bladder tuberculosis, sufferings so great that death, which comes with tardy feet, through the involvement of the second kidney, or metastases in other parts of the body, is to be longed for.

Without elaborating my argument, I would express myself as to the origin and course of the disease as follows:

1. *Tuberculosis of the kidney, not including general miliary tuberculosis, is always unilateral at the start.* Even autopsy statistics show that 50.5 per cent of cases are unilateral, while studies of the separated urines and the post-operative sequelæ show that 54 out of my 62 cases were unilateral; a percentage of 84.4 per cent. Albarran found 58 out of 64, or a little over 91 per cent, of his cases were unilateral. He has therefore been fortunate in securing his patients at an earlier stage. I consider my group of cases as an advanced one, since the average duration of symptoms had been, as I said, 3½ years when I first saw them, nearly two thirds showing bladder involvement.

2. *Tuberculosis of the kidney is practically always hæmatogenous in its origin.* There is no association between genital and renal tuberculosis by way of the genito-urinary excretory channels or by the bladder. They may coexist independently, or the infection may be transmitted by the blood.

3. *Primary tuberculosis of the bladder is rare.* In a large experience with bladder conditions in women, I have seen only one case unassociated with kidney tuberculosis.

4. Although a bladder tuberculosis cannot heal while there is a tubercular kidney above, it does respond to suitable treatment after a nephrectomy.

The operative procedures to be considered in this connection fall into several groups: Nephrotomy, partial nephrectomy, and nephrectomy, including nephro-ureterectomy and nephro-uretocystectomy. Let us glance first at the results of nephrotomy and consider its indications.

1. *Nephrotomy*, or nephrostomy, is indicated in desperate cases of pyonephrosis, whether from a purely tubercular or from a tubercular mixed infection. When the other kidney is not tubercular, nephrotomy must always be followed by nephrectomy. When the remaining kidney is gravely involved in the tuberculosis, nephrotomy should be the only operative procedure, for in such cases it often effects a remarkable temporary improvement in the patient's general condition. In four cases in which I have used it, I have seen great immediate relief of the symptoms, although the patients have all died within a year of the operation. As a preliminary to nephrectomy, nephrotomy may be of inestimable value. I have done it in this way seven times, and all of these patients are well to-day. Some of the seven were among the most unpromising-looking patients I have ever seen. Bedridden, emaciated to the last degree, running a hectic fever, with a huge pus-kidney, death seemed inevitable. Two months is usually a long enough period for such a patient to rally sufficiently to stand a nephrectomy, which had then best be done by the intracapsular method.

2. *Partial nephrectomy* for tuberculosis has up to the present time had but a small range of application. Israel reports two unsuccessful attempts which he followed by nephrectomies. Fenger tried it faithfully, and his patients all died. Bazy and Pousson have had similar experiences. Henry Morris reports a case in which he removed a tubercular kidney, and then at a later date had to take out one third of the other kidney for tuberculosis at one pole. His patient was well, when heard from several years after the operation.

A study of the specimens from my cases shows that in five the disease was localized and

limited to but one pole. The most interesting of these is a kidney which I show here. It has a double pelvis, two ureters, and a double blood-supply. The upper half of these two fused kidneys is destroyed by tuberculosis, while there is no disease at all in the lower half. Albarran has also reported a case similar to this. Such a case should have been treated by a resection, as it affords the most favorable conditions imaginable.

I do not wish to advocate that hazardous operation, a partial nephrectomy, as a substitute for nephrectomy, but I do believe that further attempts should be made in this direction, in conditions which positively forbid a nephrectomy, as, for example, where both kidneys are tubercular, and yet both are doing good work. One case in which I employed it did not prove a success, as far as a cure was concerned, for the patient died eight months later, but her life was not shortened by the operation. Such patients are now left to a distressing fate; as a rule, a rapid *exitus letalis*. Attempts in the direction of partial nephrectomy can do no harm, and there is a chance now and then of rescuing the better kidney. Let the rule be to take out the bad kidney first and then to resect the better one.

A rare condition in which this operation is clearly indicated is when one half of a horseshoe kidney is tubercular. Lennander of Upsala has reported a successful case of this kind.

3. The operation of widest application, and that which should always be done if possible, is nephrectomy. In the comparatively exceptional case of one-sided kidney tuberculosis, with the other kidney healthy, and no tuberculosis evident in the bladder or elsewhere in the body, no one, in view of the well-established facts, should dispute the imperative indication to remove the diseased kidney. Unfortunately, however, it is in this very group of cases that dilatory measures are oftenest advised. It is true, the patient often seems to be in robust health and the local symptoms appear trivial; nevertheless it must not be forgotten that no case of spontaneous healing has been demonstrated, while the diseased kidney, so long as it is harbored, is a constant menace both to the bladder and to the other kidney, and so to life itself. My test of spontaneous healing calls for a preliminary accurate and positive diagnosis of tuberculosis of the kidney, with the subsequent demonstration that the kidney has recovered and has continued to functionate, and has shown no evidence of tuberculosis over a long period after the healing is alleged to have occurred.

The operative treatment of the cases of unilateral tuberculosis of the kidney, without bladder involvement or with only slight involvement about the ureteral orifice, and without serious tuberculosis elsewhere, promises a great deal, as my cases teach. Excluding two patients, operated on too recently to be reported, I have had twenty other cases of nephrectomy in which this favorable condition was present. All in my list, without exception, remain in perfect health over periods extending from two to ten years. *In view of these results, I cannot too strongly urge that the proper time to do the nephrectomy is as soon* as convenient after the diagnosis has been made. When the disease is further advanced, the difficulties multiply rapidly; the likelihood of disease in the second kidney is increased; grave bladder troubles are present, rendering topical diagnosis very difficult or even impossible; perirenal changes occur, greatly adding to the seriousness and dangers of the operation. Out of thirty-six of these advanced cases, I have had four primary deaths, and five more within periods of from two months to four years. The bladder conditions have in some cases demanded treatment for months before healing was effected.

In considering the limits of nephrectomy, it is convenient to take up the complications met with in my cases, and to consider them separately as regards their bearing on this question. I will take up in order tuberculosis of the bladder, tuberculosis of the genitalia, tuberculosis elsewhere in the body, and tuberculosis of the second kidney, complicating tuberculosis of a kidney upon which nephrectomy is proposed. I will then consider certain affections of the second kidney which are not tubercular, but whose existence complicates the situation.

1. Involvement of the bladder, far from being a contraindication, is a marked indication for nephrectomy. As long as the tubercular kidney above pours down infectious material, the bladder cannot be healed, whereas it often

yields promptly to simple treatments after the nephrectomy.

My series includes thirty-six cases where extensive bladder tuberculosis was present. Out of this number, there were four deaths immediately dependent upon the operation (a mortality of eleven per cent); three more died within a year, one after two years, and one after four years, making a total of nine deaths, or twenty-five per cent mortality. Eighteen patients, or fifty per cent of the total number, have been entirely cured for periods of longer than two years and up to twelve. Seven patients, all of whom have been operated on within a year and a half, still have residual bladder trouble; one has had it for two years, and one patient for over four years. The last two patients have not taken regular treatment; one of them has been through a pregnancy, and both may be considered as having neglected themselves.

Excluding the cases where bladder trouble is still present, the total mortality for this group of cases has been thirty-three per cent; unfavorable indeed compared to the simple cases with no mortality, but still excellent, in view of the certain death of all without operation. It is of interest to consider the causes of the primary deaths. In two of the four cases it was from anuria. These cases were quite similar. In each, one kidney had been entirely destroyed and changed into a fibro-fatty mass, and in each, the other kidney was tubercular. Both patients were in extremely bad general condition. The bladders were so small, contracted, and ulcerated, that a separate study of the urines of the two kidneys was found impossible. One patient had acquired in the active kidney a sudden pyæmic infection, which led to the nephrectomy.

The other of these two cases had a pelvic tubal tuberculosis in addition to the double renal involvement. The abdomen was opened, and the kidneys and ureters palpated. On the side which was entirely inactive, the kidney and ureter were of normal size, and the ureter of normal appearance, while on the other side the kidney was twice the normal size, and the ureter thickened. The situation was desperate, and I do not know how the ill result could have been avoided.

One of the remaining two cases of the four primary deaths died of peritonitis, and the other of what seemed a general infection, no autopsy being made.

Of the secondary deaths, three of the five were from involvement of the remaining kidney. One of them, known to have tuberculosis of the second kidney at the time of the operation, died within two months; the other two, with clear urines of the second kidney, died within two and four years, respectively, of the operation. Of the two remaining deaths, one was from heart failure, occasioned by an operation to remove the tubercular ureter which had been left in place, and one from an unknown cause, forty days after her operation.

2. Tuberculosis of the genitalia is a more frequent complication in men than in women. My experience with this complication is limited to the latter, so far as this associated localization is concerned. I have had five cases in this category, of which three have entirely recovered; one has only recently been operated upon, and one, as already cited, died from the operation. In these cases, the tubercular genital organs must always be removed, as well as the kidney. Rovsing has recently reported ten cases of epididymal and renal tuberculosis which he has cured, showing that the results in men are also good in this class.

3. Tuberculosis elsewhere in the body, if not extremely active, is no contraindication to operation. I have had, in all, eighteen such cases of coexisting tuberculosis, pulmonary, joint, or glandular. After nephrectomy and under general treatment, thirteen have recovered, and have been apparently entirely well for over two years. Three have been operated upon within two years and three have died.

4. Tuberculosis of the second kidney. In cases where the worse kidney is not secreting at all, or very little, its removal seems to benefit the sounder kidney, and to help the general condition remarkably, if only temporarily. This is true especially when the worse kidney is actively manifesting the disease. I have had seven such cases, when the nephrotomies are included, and I must admit that it has been but a palliative measure in my hands. Except where the patients are rapidly going down from pyonephrosis, I think that the prolonga-

tion of life it affords can be measured in months, and the relief of symptoms is not great. As already stated, the relief of a pyonephrosis, even when the other kidney is tubercular, often leads to a remarkable temporary gain in general condition.

5. The presence of hyalin and granular casts and albumin in the urine from the second kidney does not contraindicate operation. In four cases out of five, they disappeared shortly after the nephrectomy, and in the fifth case, where they persisted, and demonstrated a true nephritis, the patient has been actively engaged in school-teaching for seven years.

6. In three of my nephrectomies there has been a well-defined colon bacillus pyelitis of the non-tubercular kidney. These patients are alive thirteen, seven, and four years, respectively, since their operations.

It is evident from these considerations that nephrectomy has a wide range of utility in the treatment of renal tuberculosis. I cannot here go into the operative and other treatments of tuberculosis of the bladder. They demand a consideration as wide as that given the condition in the kidney in this paper. In general, it is a fact that the cases in which the ureters are removed respond more quickly to bladder treatment than those in which they are left in, and the incisions close sooner. If the entire ureter is thickened and tubercular, provided the patient's condition warrants it, a complete ureterectomy should also be performed, an operation which I have called nephro-ureterectomy. In most of my cases there was thickening and tenderness of the vesical end of the ureter, showing a tuberculosis at that level. This was found in case after case, where the upper part of the ureter was normal. In most of these patients, after a nephrectomy, the vesical condition healed spontaneously. The ureters in tuberculosis cases, when not removed, should be divided with a Paquelin cautery and sunk. One of the most obstinate sinuses I have had was in a case where I sutured the ureter in the lower angle of the incision, according to Israel's recommendation.

In pyonephrosis requiring a preliminary nephrotomy, the intracapsular method has given excellent results, and although the sinus may persist for several years, it will ultimately close. I have had one case which remained open for four years.

The statistics which I have presented to you clearly demonstrate that in any satisfactory consideration of tuberculosis of the kidney, it is necessary to classify and group the cases, in order to obtain a just estimate of the operative results. I find that the literature of the subject is not of great assistance in securing these comparative statistics. In most of the statistical reports there is no more detailed consideration than the statement of the primary mortality. Taking the cases as they run, the primary mortality will depend upon the skill of the operator, as well as upon the severity of the cases with which he has to deal. There can be no question but that, given earlier cases and simpler conditions, the primary operative mortality will drop many per cent.

Grouping my cases all in one, I have had 57 nephrectomies, with 4 deaths; giving a primary mortality of 7 per cent.

In 1900, Pousson collected 600 cases reported in the literature, and found 21.33 per cent primary mortality.

Albarran reports 57 cases and 2 deaths; 3.5 per cent.

Rovsing, 47 cases, 3 deaths; 6.3 per cent.

Israel, 70 cases, 12 deaths; 17.8 per cent.

Kümmell, 42 cases, 6 deaths; 14.3 per cent.

Pousson, 23 cases, 3 deaths; 13 per cent (personal).

These statistics as to primary mortality are, of course, of great value, but if the class of cases operated upon is clearly defined, we then have a better basis for estimating the dangers of the operation in a given case. They give us, also, but a slight hint as to the ultimate curative power of the operation. Kümmell states that 32 out of 43 of his cases, or 74 per cent, were living some time after the operation. Albarran was able to follow 39 out of 53 of his cases, and stated that 5 of these 39 died of further development of tuberculosis. Pousson, in his collection of statistics from the literature, estimates that 50 per cent are living after several years. No mention is made in any of these statistics as to whether the patients were really well, and this, of course, is an essential factor, since it is possible for a patient to live several years with only one kidney, and that a tuber-

cular one. My own collection is unique in this respect, that every patient has been followed, not one having been lost sight of. From all who are now living, reports have been secured and careful urinary examinations have been made. Many have been seen recently, and cystoscopic examinations have been made, and only those cases in which the urine has been found normal, and where the patient has had no symptoms of disease, has a cure been registered. I think, for the future, that all reports of tubercular kidneys treated surgically should be precise in this respect.

In conclusion, I would again emphasize the hopelessness of medicinal and hygienic measures alone, and therefore emphatically condemn the waiting treatment of tuberculosis of the kidney.

I wish, therefore, particularly to urge a prompt early operation. We have ceased to discuss the advisability of operating promptly in acute appendicitis, and yet here is a condition more imperatively demanding operation, and that, too, an early operation, a condition even more hopeless in its outcome than any given case of acute appendicitis. When the profession at large comes to realize this fact, and calls upon the surgeon to assume the early treatment of renal tuberculosis, then the results of surgical interference will improve *pari passu*.

VAGINAL SECTION AS AN OPERATION OF CHOICE[1]

By HENRY T. BYFORD, M.D., Chicago

THE principal reason for the performance of vaginal section for diseased conditions of the uterus and appendages has heretofore been the great danger attaching to abdominal section. It was discovered about twenty years ago, in Switzerland and Germany, that the uterus could be removed per vaginam with much greater safety than per ventrum. About the same time, it was demonstrated in this country that the ovaries could, in properly selected cases, be more safely removed in the same way. Not long afterward it was discovered in France that the uterus and adnexa could also be removed in bad cases of pyosalpinx more safely by way of the vagina.

But these facts had hardly been demonstrated before the statistics of abdominal section began to improve so rapidly that there is to-day but little difference in the relative mortality. The improved antiseptic technique and the greater facility for dealing with the pathological conditions offset the greater exposure of the peritoneal cavity, which becomes of less moment than the almost insurmountable difficulties and delays sometimes connected with vaginal section. And as the technique of the latter method requires special training and much practice for its mastery, the abdominal is naturally the operation of choice among general surgeons. However, the ultimate rule of practice must be the selection of the method that secures the best results to the patient, both with regard to mortality and morbidity.

The cases adapted to vaginal section are those in which the parts affected are more accessible from below; viz., those connected with prolapse or retroversion of the uterus, or with prolapse of the ovaries, or with adhesions low down in the pelvic cavity, and cases of hysterectomy or myomectomy for small fibroids, as well as hysterectomy for other small neoplasms and malignant diseases of the uterus. The removal of a fibroid uterus larger than a fœtal head at the sixth month is apt to be a prolonged operation, and to involve such a loss of blood and such depression from the long-continued anæsthesia, that it may be considered more dangerous than a quickly performed abdominal section. Ovarian cystomas too large to be pushed down into the pelvis should not, as a rule, be attacked per vaginam, because the danger from an unmanageable complication is greater than that which belongs to abdominal ovariotomy.

[1] Read before the American Surgical and Gynecological Society, Baltimore, December 11, 1906.

In those cases in which the uterus is not to be removed, the peritoneal cavity may be opened by an incision through the posterior fornix into the cul-de-sac of Douglas or through the anterior fornix between the bladder and uterus. When the work can be done equally well by either route, the posterior incision is preferable, as being simpler and easier. This posterior route is nearly always the better one to employ when the uterus is retroverted or readily retrovertible and the cervix can be drawn well down near the vulva. The appendages, as well as small tumors low down on the posterior uterine surface, can be drawn into view and near to the vaginal entrance, and removed or treated, as may be advisable, and the incision be completely closed with three or four catgut sutures.

The anterior incision has the advanatge of enabling the operator to deliver the fundus into the vaginal entrance, and also to draw the ovaries and round ligaments forward within easy reach. Among its disadvantages are, a complicated wound which cannot always be as satisfactorily closed as the posterior median incision, and the presence of the fundus uteri in the vagina to interfere with our manipulations, and perhaps become more or less injured. The fundus, of course, is not always drawn into the vagina; but in dealing with diseased appendages this is about its only advantage over posterior colpotomy. Another disadvantage is, that the statistics are not as good for operations upon the ovaries as by the way of the cul-de-sac of Douglas. For shortening the round ligaments and anterior fixation of the uterus and the removal of small subserous myoma from the anterior uterine wall, the anterior incision is, however, the method of choice.

There are sometimes special reasons why abdominal incisions should be avoided. In such cases the field of vaginal section may be extended by removing the uterus; or, if it be desirable to preserve the uterus, by a lateral coloptomy in connection with an anterior or posterior colpotomy, or with both. By extending the anterior or posterior vaginal incision, whichever has been made, around one or both of the lateral fornices, or connecting a posterior with an anterior incision, we can, by ligating and severing the uterine artery, or by skirting or shaving the edge of the cervix with scissors or a spud, separate the broad and sacro-uterine ligaments from the cervix. This not only gives space for the introduction of the whole hand into the peritoneal cavity, but enables us to employ intraperitoneal illumination. Or, by extending the anterior incision around both lateral fornices and separating the bases of both broad ligaments as well as the sacrouterine, from the cervix, we can get about as much room as if the uterus were removed. In fact, I have extended the vaginal incision completely around the cervix as for vaginal hysterectomy, separating the bases of both broad as well as the sacrouterine ligaments and the bladder, and afterwards sutured the parts back in their original relationship, without any bad results. Through the opening thus made the torn or perforated rectum can be successfully sutured. Thus we may say that all sorts of operating can be done through the vagina.

When, however, the cervix is held up and back near the sacrum, and adhesions can be felt by bimanual examination to extend high up in the pelvis; when there is some other condition in the abdominal cavity that requires attention; when there are tumors that extend high above the pubes; and when the vagina is poorly developed or has undergone considerable senile atrophy, — the abdominal route is preferable, even though a vaginal section is possible.

The personal equation must also have some weight in the choice of methods. One should do that which he can do best, and avoid that for which he is not trained. The gynecologist who has had an extended experience in vaginal section, and who has learned to diagnose pathologic conditions within the pelvic cavity with considerable accuracy, naturally knows how to do things from below, and whether they can be done with certainty and celerity. He rightly chooses the vaginal route in many cases. But a general surgeon, who has had but little training and experience in vaginal work, will more often choose the abdominal route, and in so doing will give the patient the best chance.

Unfortunately, some patients still have a great fear of the abdominal incision, and will

insist that the operation be done by way of the vagina. It is needless to say that the operator should not allow her to force this method upon him, unless his experience is such that he knows that he can thereby give her as good a chance for recovery.

With regard to the fate of vaginal section, while its utility is diminishing, and will undoubtedly diminish with the perfection of the technique of abdominal section for pelvic conditions, there will always be a place for it, and the time has not yet come when a special training in gynecologic surgery can be considered as superfluous. In other words, the gynecologist cannot yet be deprived of the right to live a little longer. But, as methods become simplified and indications better defined, special technique will be of correspondingly less importance.

In conclusion, I will venture to make the general statement that vaginal section will retain its place for the treatment of cases that can be completed satisfactorily and quickly by it, and for those in which, on account of inaccessibility or special contraindications, abdominal section is contraindicated.

TUBERCULOSIS OF THE KIDNEY[1]

By CHARLES P. NOBLE, M.D., PHILADELPHIA
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IN no other part of the general subject of tuberculosis has greater progress been made in our knowledge than in tuberculosis of the kidney. As recently as ten years ago the teaching of recognized authorities concerning tuberculosis of the kidney was not only very incomplete, but, as subsequent investigations have shown, it was also largely erroneous as to the mode of invasion, the gravity of the lesion, and the nature of its treatment. I shall not attempt a systematic study of the subject, but wish to present my own experience, more especially with nephrectomy for tuberculosis, and to discuss certain problems which enter into the general question from the standpoint of that experience.

My first nephrectomy was performed in 1895, at which time the operation was relatively a novelty. At that time it was believed that tuberculosis of the kidney was frequently if not usually bilateral, and it was held that when it was not a part of a generalized tuberculosis, it was usually a secondary condition due to tuberculosis of the bladder with ascending infection. For these reasons the field for curative treatment was believed to be a very small one, and the mortality of nephrectomy for tuberculosis was estimated as at least 33 per cent.

The views current ten years ago were almost exclusively the result of the teachings of pathologists, based upon the findings at autopsy, and therefore were based upon the conditions found after the disease had run a fatal course. It is easy to understand why pathologists formed erroneous views from the evidence presented at autopsies. When the kidney involvement was moderate, the disease was usually secondary to primary tuberculosis of the lungs; and when the kidney involvement was primary and extensive, the disease had run its course and the ureter and bladder were found to be involved. The pathologists inferred that the tuberculosis of the bladder was primary, and the infection of the ureter and kidney secondary, from the analogy of the infection of the ureter, pelvis of the kidney, and kidney, with other micro-organisms. As is well known, when not due to pyæmia or to bacteriæmia, ureteritis, pyelitis, and pyelonephritis are commonly due to ascending infection from a primary cystitis. No evidence has come under my observation that tuberculosis of the kidney in women is ever due to ascending infection. On the contrary, in fifty per cent of the cases the bladder was but little if at all infected, and in all of the cases the ulceration of the bladder or cystitis has under-

[1] Read before the Southern Surgical and Gynecological Association, Baltimore, December 11, 1906.

gone either spontaneous recovery or has recovered after local treatment subsequent to the removal of the primary focus of the disease in the kidney by nephrectomy. Cystitis existing as a complication of tuberculosis of the kidney is usually due to infection from the pus entering the bladder through the ureter. In a considerable percentage of the cases, however, there is a mixed infection, and the cystitis is due, not to tubercular infection, but to infection from some other organism.

My experience does not warrant me in expressing an opinion concerning the relative frequency of primary tuberculosis of the bladder in men. The fact that tuberculosis of the epididymis and of the vesicula seminalis is not rare would indicate a more substantial basis for the occurrence of primary tuberculosis of the bladder with ascending infection in men; but it is interesting to find that the more recent studies of this subject show that the older view concerning the frequency of primary tuberculosis of the bladder is almost if not quite as erroneous with reference to men as it has proved to be with reference to women.

Diagnosis. The diagnosis of tuberculosis of the kidney is usually difficult, except in late cases in which the kidney is much enlarged by pus contained within its capsule, or in which the presence of a perinephritic abscess, together with the history of the patient, indicates the probable nature of the malady. In but a minority of the cases were the patients referred to me with a diagnosis of disease of the kidney; in the majority of them the diagnosis was cystitis. The first symptom, in most cases, is increased frequency in urination. This usually persists and becomes more marked. Examination of the urine shows the presence of pus, and the urine is usually acid. Frequent urination and persistent pyuria with acid urine is a combination of signs which should always raise the suspicion of tuberculosis of the kidney. The suspicion is increased if the patient shows evidence of failing nutrition and discomfort in the region of one or the other kidney. Blood in the urine is a frequent symptom, usually microscopical in amount, but occasionally sufficiently free to cause a definite hæmaturia. Pain in the kidney is rarely the first evidence of tubercular involvement, and when present is usually one of the evidences of extensive involvement of the kidney. Intermittent pain, more or less colicky in character, is at times due to partial obstruction of the ureter with pus, and at other times to stricture of the ureter with accumulation of urine or pus above the stricture. Rarely, the presence of a mass in the region of the kidney, with or without local pain, is the first sign to draw attention to the kidney involvement.

The symptoms and signs already given are sufficient to raise the presumption of the presence of tuberculosis of the kidney, especially when the patient has a tubercular family history; but a positive diagnosis, except in the most advanced cases, can only be made by establishing the tubercular character of the lesion. For a time it was thought that this could be determined by finding tubercle bacilli in the urine. Considerable experience with the method has convinced me, in common with many others, that this method of diagnosis is fallacious and unreliable. A negative report may mean that the technique employed by the examiner was poor, or that his patience was soon exhausted; and a positive report brings with it a doubt as to whether the examiner has confounded the smegma bacillus with the tubercle bacillus. Until means of differentiating between these two organisms by bacterioscopic methods is discovered, the supposed finding of tubercle bacilli in the urine is not to be considered, by any means, as positive evidence of tuberculosis.

Since my fifth nephrectomy in 1899, I have relied upon the method of injecting the urine into guinea-pigs. One of the chief objections to this method is the time which must elapse between the injection and the diagnosis, usually five or six weeks. It has the great advantage, however, of determining the presence of tubercle bacilli, even in early cases of tuberculosis of the kidney. Like all laboratory methods, it is open to errors in technique. In the five cases in which a positive report has been received, the diagnosis was confirmed by subsequent operation and pathological examination; and of numerous cases in which a negative report was furnished, in none has the subsequent history of the patients shown an error. I am familiar, however, with one case in which a positive

report was furnished through a mistake. In this case, the surgeon was not satisfied with the diagnosis, and sent an additional specimen of urine, from which a negative report was received. The subsequent history of the patient proved that tuberculosis did not exist. In this case, either the guinea-pigs were mixed or a guinea-pig already infected with tuberculosis was injected, and thus an erroneous report was furnished. At the present time a positive report from the injection of urine obtained by ureteral catheterization is the most reliable evidence of tuberculosis of the kidney.

Cystoscopy furnishes evidence of value. The trained examiner is able to form a pretty accurate conclusion as to whether the extent of the cystitis or the ulceration in the bladder is sufficient to account for the amount of pus found in the urine. The appearance of the ureteral orifices is also indicative of the condition of the kidneys. A normal ureteral orifice is incompatible with a long-standing pyelonephritis with discharge of pus through the ureter. As a rule, the ureteral orifice is œdematous, congested, or positively inflamed, and the region of the ureter is the site of either inflammation or ulceration. I do not believe that cystitis and ulceration associated with tuberculosis of the kidney can be differentiated from that due to infection from other organisms by means of the cystoscope alone.

Catheterization of the ureters, a study of the urine obtained from each kidney, and the injection of urine from each kidney into guinea-pigs, are the most reliable means of diagnosis at our disposal. The objection has been made to this method of diagnosis, that it involves the possibility of infecting the previously healthy ureter and giving rise to ascending infection. In no case has this occurred in my experience, either in tubercular or non-tubercular cases. To avoid, or at least to lessen, the risk of infection, the bladder should be irrigated immediately before the examination; and when the supposedly healthy ureter is catheterized, the orifice of the ureter should be washed with a pledget of cotton immediately before inserting the ureteral catheter. When ureteral catheters are inserted to obtain urine for examination, it is unnecessary to pass them more than one or two inches, and in the supposedly healthy ureter it is best to be satisfied with a moderate insertion of the catheter.

Condition of the Bladder. The condition of the bladder before operation and the result of nephrectomy upon the condition of the bladder is one of the most important subjects connected with tuberculosis of the kidney. From the patient's standpoint, the irritability of the bladder, entailing the necessity for frequent urination, is the chief trouble for which relief is sought. Of the ten patients upon whom I have performed nephrectomy for tuberculosis, in three the bladder was not examined. Two of these were emergency cases, seen in a neighboring town, whose condition was such as to demand operation on the day when they were first seen. In the third patient the bladder was not examined, as the operation was undertaken with a diagnosis of movable kidney. In one of the remaining seven cases, bladder symptoms were not complained of, although there was some pus in the urine. A cystoscopic examination showed that the bladder was practically normal. Of the remaining six cases, cystitis was present in all, and ulcerations, more or less marked, were present in the region of the ureters and trigone in five.

The cystitis and ulceration underwent a spontaneous cure in five of the six cases. In one case it was necessary to make topical applications to the ulcers of silver nitrate through the cystoscope. In one case a vesicovaginal fistula had been made prior to the nephrectomy, and it was not closed until the ulcers healed. In two of the patients, irrigation and distention of the bladder by gravity was practised to increase its capacity. This experience indicates that after nephrectomy for tuberculosis a secondary cystitis will usually undergo a spontaneous cure, which may be hastened in certain cases by irrigation, topical applications, or drainage. In the graver cases complicated by extensive ulcerations in the bladder, the formation of bands or septa and a permanent loss of the full capacity of the bladder are to be expected in some of the cases.

Operation. Nephrectomy alone was performed in seven of the ten cases. Nephroureterectomy was performed in two cases, and in one the upper two inches of the ureter was removed, together with the kidney. Lumbar

nephrectomy was performed in the last nine cases. In the first, nephrectomy by abdominal section, with a T-shaped incision, was the method elected. In this case the tubercular abscess of the kidney was complicated by a large perinephritic abscess which had been drained four weeks before the radical operation.

Nephrectomy is a much simpler operation than nephroureterectomy, and is the operation of choice, unless the ureter is extensively diseased. It must not be forgotten, however, that inflammation of the ureter complicating tuberculosis of the kidney is frequently simple and not tubercular. In two cases, when the ureter was known to be diseased, ureterectomy was not performed, because of the prostrated condition of the patient. In one, the upper two inches of the ureter was removed, and in the other, no effort was made to remove ureteral structure. The most urgent indication for ureterectomy is tubercular involvement of the ureter with a stricture toward its vesical end. To leave the ureter under such circumstances would almost inevitably result in a permanent fistula.

The influence of nephrectomy, as compared with nephroureterectomy, in the healing of the wound is suggestive. Of the ten operations, six were followed by sinuses in the lumbar wound, which remained open for varying intervals of from four to fourteen months. Both patients upon whom nephroureterectomy was performed developed sinuses. The sinus in the first case closed in about four months. In the second case, a sinus formed twice, and closed finally about nine months after the nephroureterectomy. Of the remaining cases, the two in which the sinus remained open the longest were those in which the tubercular process was most extensive. In one there was a perinephritic abscess with tubercular infection of the entire renal region. In this case, silk ligatures were used (1895), and the sinus did not close finally until all the ligatures had been discharged after about one year. In the second, a tubercular ureter was left in because of the feeble condition of the patient. This sinus closed after fourteen months.

It is clear that a strong tendency exists for the persistence of a sinus following nephrectomy, whether or not all apparently diseased structure, including the ureter, is removed. This is doubtless due to the tubercular infection of the perirenal tissues, recovery from which, like that from tuberculosis in general, is due to improvement in the patient's nutrition.

Results. Nine of the operations were done upon women and one upon a man; two were colored and eight were white. Six of the patients were in bad condition, three in fair condition, and one in good condition at the time of operation. The man died, not from the nephrectomy, but, some six weeks later, presumably from involvement of the lung, and in the course of the tubercular disease. The nine women not only recovered from the operation, but all of them are in good health, also, at the present time. Most of them have gained from twenty to forty pounds in weight. All have been heard from during the preparation of this paper, and report themselves as well, with the exception that three have bladder symptoms. In two, the bladder will not hold more than six ounces, and when this amount has accumulated, urination is necessary. The third (operated upon in February of this year) is still obliged to urinate frequently. She has, however, improved locally, and has gained forty pounds in weight.

The Incision for Nephroureterectomy. In the first nephroureterectomy the incision recommended by Kelly was used. In the second, after freeing the kidney and ligating and detaching its vessels, the ureter was detached as far down as feasible through the lumbar wound, and then the patient was turned upon her back, the kidney being permitted to lie upon the operating-table, attached to the ureter. An incision was now made just internal to the outer border of the rectus muscle, that is, just internal to the semilunar line. The incision was made through the rectus rather than through the external oblique, internal oblique, and transversalis, to avoid permanent damage to the abdominal wall. It is well known that the usual incision through these three muscles must lead to permanent injury of the abdominal wall. At the best, the muscles are tied up in a scar, which is more or less uncomfortable when the abdominal muscles are brought into play, and at the worst, ventral hernia is invited. By making the incision through the rectus muscle,

when the wound is closed the fascias can be overlapped, thus preventing hernia; and, in addition, the abdominal muscles are not tied up in a cicatrix, which must permanently interfere with their normal freedom of motion. The ureter can be reached as easily through this incision as through the one heretofore employed.

In each of the ten cases of nephrectomy forming the basis of this paper, the tuberculosis of the kidney presumably was the primary lesion, and in only one (the colored man who died some six weeks after operation) were the lungs involved. As seen by the surgeon, tuberculosis of the kidney is usually unilateral. When both kidneys are involved, the tuberculosis of the kidneys is usually a part of general tuberculosis, or the second kidney becomes involved at a much later period than the first. In either case, the general condition of the patient is usually such that he is not referred to the surgeon.

The excellent results secured in this series of cases may be regarded as typical of what can be accomplished in the cure of tuberculosis of the kidney by operation, if nephrectomy is performed reasonably early in the course of the disease. To accomplish the most satisfactory results, nephrectomy must be performed before secondary involvement of the bladder has become extensive and before the disease has been disseminated throughout the body. These fundamental facts should be appreciated by the family physician. Were it feasible for him to make a positive diagnosis early in the disease, he should be urged to do so, and to advise his patients to submit to early operation. Owing to the difficulties of positive diagnosis early in the disease, it is unreasonable to expect the physician who is not trained in the making of cystoscopic examinations and the catheterization of the ureters to make a positive diagnosis in early cases. He should, however, be on the alert to suspect the presence of tuberculosis of the kidney whenever he has to deal with a persistent pyuria with acid urine, and when the usual treatment for cystitis pursued for a reasonable time fails to effect a cure. In these and other cases of pyuria beyond his diagnostic resources, he should call in expert assistance, in order that the diagnosis may be cleared up.

TABLE OF NEPHRECTOMIES FOR TUBERCULOSIS OF THE KIDNEY

Name and Date	Age	Condition	Diagnosis	Operation	Ureter	Union	Result	Family Physician	Bladder
Mrs. B. 10/17/95	34	Anæmic, prostrated.	Tuberculosis of left kidney; kidney structure destroyed, perinephritic abscess; bladder not ulcerated.	Left nephrectomy; incision of perinephritic abscess, Oct. 17, 1895.	Not removed.	Sinus closed after one year.	Cured.	Klein.	Not involved.
Miss E. 5/16/96	37	Septic, bad.	Tuberculosis of left kidney; bladder ulcerated.	Left nephrectomy.	Not removed.	Primary.	Cured.	Minnich.	Cystitis and ulceration of bladder. Recovered spontaneously.
Mrs. L. 12/3/96	31	Fair.	Tuberculosis of right kidney; bladder not examined.	Right nephrectomy.	Upper two inches removed.	Sinus closed in six months.	Cured.	Applebach.	Not involved.
Mrs. F. S. 4/14/99	37	Septic.	Tuberculosis of left kidney; bladder not examined.	Left nephrectomy.	Not removed.	Primary.	Cured.	Sharpless.	Not involved.
Miss E. B. 7/24/99	30	Bad.	Tuberculosis of right kidney; bladder ulcerated.	Right nephrectomy.	Infiltrated, but not removed, on account of patient's condition.	Sinus closed in fourteen months.	Cured.	Jones.	Ulceration treated topically through cystoscope. Cystitis treated by irrigations. Recovery, with the formation of a band or septum across bladder. Capacity of bladder increased from 60 c.c. to 180 c.c.
Mr. F. 4/9/01	44	Septic, very bad.	Tuberculosis of right kidney; bladder not examined.	Right nephrectomy.	Not removed.	Sinus.	Died of pulmonary tuberculosis.	Sharpless.	Not involved.
Miss C. 10/16/01	27	Good.	Tuberculosis of right kidney; bladder not ulcerated.	Right nephrectomy.	Not removed.	Sinus closed in six months.	Cured.	Robertson.	Mild cystitis. Recovered spontaneously.
Miss B. 10/20/02	27	Bad.	Tuberculosis of left kidney; bladder ulcerated.	Left nephrectomy.	Not removed.	Primary.	Cured.	Clark.	Ulceration healed, but bladder remains contracted.
Miss C. 6/15/05	22	Fair.	Tuberculosis of left kidney; bladder ulcerated.	Left nephrectomy.	Removed.	Sinus closed after four months.	Cured.	Jones and Kyle.	Ulceration healed spontaneously.
Miss. B. 2/14/06	25	Fair.	Tuberculosis of left kidney; bladder ulcerated.	Left nephrectomy; cystotomy. Fistula subsequently sutured.	Removed; stricture above vesical wall.	Primary. Sinus formed twice subsequently.	Cured.	Ingalls and Stanton.	Ulceration healed after drainage and irrigations. Bladder remains contracted.

HYSTERECTOMY AND REMOVAL OF THE BODY OF THE UTERUS[1]

The Comparative Advantages and Disadvantages of Hysterectomy and Removal of the Body of the Uterus

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As the study of uterine neoplasms progresses, and the after-history of patients operated on for such growths is more carefully considered, we are confronted with the fact that malignant disease of the cervix uteri is discovered comparatively frequently after the body of that organ has been removed. It is found that an important proportion of cases of tumors, thought to be simple fibromata or myomata, have in reality taken on malignant degeneration or are complicated by a malignant degeneration of the corporeal endometrium. Carcinoma of the cervix has been detected within a few days after removal of the body of the uterus, showing clearly that it existed at the time of the supravaginal amputation. In innumerable instances, removal of the cervical stump has been done at the same sitting, prompted by discovery of malignancy in the uterine body just removed. Not a few observers have seen cases in which fibromata have existed for years, the patients declining operation, and cancer of the cervix develop subsequently, with a fatal result. I have seen two such cases. One of them I saw first in consultation in 1886, and again a few months before her death in 1902. The fibroid mass extended to about two inches above the umbilicus, remaining practically stationary throughout the sixteen years. No doubt exists as to the necessity for the removal of the cervix with the body in malignant disease of the uterus in its early stages. The relative facility of supravaginal amputation of the uterus, compared with total extirpation, has created a very decided preference in favor of the former procedure. If this prejudice is to be overcome, it will be done only by overwhelmingly proving the necessity therefor — a result I cannot hope to effect by what I offer in this paper. I can only invite the attention of this body of surgeons and gynecologists to the facts connected with the proposition. To do this, several points suggest themselves for consideration. They are:

1. The relative frequency of cancer of the cervix, with or without fibroids of the uterus.
2. The relative frequency of cancer of the body of the uterus independently and as a complication of fibroids in that organ.
3. The ætiological relation of fibroids of the body of the uterus to malignant diseases of it.
4. The causative relation of fibroids of the cervix and cancer of it.
5. The existence of other conditions, such as infections, catarrh, or laceration of the cervix, as a possible indication for extirpation of the cervix.
6. The malignant degeneration of uterine fibroids.
7. The difficulty of ascertaining, at the time of or before operation, whether such degeneration is present.
8. Malignant degeneration of the cervix subsequent to removal of the uterus.
9. The age of the patient, and her family history as to malignancy.
10. The comparative mortality of supravaginal amputation and total excision of the uterus.
11. The relative ease of the two operations.
12. Fatal points in the technique of the former, in addition to prompt removal of the cervix.
13. The dangers to surrounding structures in and special complications of hysterectomy.

That cancer of the cervix occurs more frequently with uterine fibroids than without them, has not been proven. This subject has received careful study by a few observers, all of whom have declared that no practical difference in such frequency exists. The same is not to be said of cancer of the body of the uterus.

Admitting that the presence of fibroids in the uterine body has no influence with developpment of cancer of the cervix, valuable infor-

[1] Read at the nineteenth annual session of the Southern Surgical and Gynecological Association, Baltimore, Maryland, December 11, 1906.

mation from statistical tables on the complications of fibroids is available to demonstrate the remarkable increase in the frequency of cancer of the body of the uterus as compared to its existence in the cervix as such complication.

The percentage of cases of cancer located in the body and the cervix are generally estimated by many competent observers as 10 per cent and 90 per cent. Williams (9) found but 4 cases of cancer of the body of the uterus to 156 involving the cervix in 160 consecutive cases of uterine cancer coming under his observation. Winter (10), in studying 2,331 cases occurring in Germany, finds the proportion was 1 to 15. Robb (7) found that of 42 cases of cancer of the uterus admitted to Lakeside Hospital, 6 of them were of the corporeal variety — a proportion of 1 to 6. Noble suggests that the proportion of cases of cancer of the uterus to those of cervical cancer is about 1 to 10. It is likewise conceded that the ratio is markedly changed in the presence of uterine fibroids. C. P. Noble found, in a study of 2,274 cases of uterine fibromata, that cancer of the body was found in 45 and of the cervix in 16 of them. This changes the percentages to 74 in the body and 26 in the cervix. The proportion of cases of cancer of the body of the uterus to those of cancer of the cervix is thus shown to be increased 26 times in the presence of fibroids. J. B. Sutton (8) had, in 500 cases of fibroids of the uterus, 63 that were more than 50 years of age. In 8 of the 63, cancer of the body of the uterus was microscopically proven, and in 2, primary cancer of the Fallopian tube was noted. He has had 23 cases of cancer of the body of the uterus, in 8 of which fibroids were present as a complication. Piquand (5), quoted by Sutton (8), collected 179 cases of cancer of the body of the uterus complicated by fibroids, and had, in 1,000 cases of fibroids, cancer of the body of the uterus as a complication in 15.

In Noble's report of 2,274 cases of fibroids, sarcoma was a complication in 34 cases, making a grand total of 95 cases — more than four per cent — of malignant disease existing as a complication.

From these figures one cannot avoid recognition of the existence of a strong influence in fibroids of the uterus as a cause of malignant degeneration of that organ.

The ætiological relation between fibroids of the cervix and cancer of the cervix has not been established. Nor am I aware that any systematic study of this subject has been projected. Fibroids of the cervix are so rare, and cancer of this portion of the uterus so common, comparatively speaking, that probably this relation, if it exists, is quite trivial. From analogy it might be considered as a working hypothesis for such investigation. That fibroids of the body exert any such influence on cancer of the cervix has not been even conjectured.

There are other conditions of the uterus that may exist, even quite commonly, that may have a deciding influence on the advisability of removal of the cervix when the body is to be removed. These are, notable laceration, marked cystic degeneration and marked hyperplasia of the cervix, infections of the uterus, especially tubercular, and chronic catarrh of either the cervix or the whole endometrium. To leave in a badly degenerated cervix after removal of the body of the uterus is to invite malignant degeneration of it.

Malignant degeneration of uterine fibroids is a subject of great importance, and is being studied very thoroughly. The time has come when removal of uterine fibroids does not end the responsibility of the surgeon. He must know the condition of such uterus and growths, both macroscopically and microscopically. We are very deeply indebted to C. P. Noble, Frederick, Hunner, Scharlieb, MacDonald, Winter, Webster, and a few others for emphasizing this subject and offering such positive *data*. They have convinced the profession that early degeneration of uterine fibroids, benign or malignant, cannot be determined by symptoms, any more than uterine cancer can be. Before operation, the discovery of malignant degeneration of fibroid uterine tumors, and of the mucosa adjacent, is, in a majority of cases, impossible.

Even uterine scrapings may not include the portion of the mucosa located on shelves caused by submucous fibroids projecting markedly into the uterine cavity. It is in such portions that the mucosal irritation and hypertrophy is most marked, and, according to many observers, the malignant growth is most apt to begin. Even at the time of operation the discovery of malignant degeneration of the uterine fibroids

cannot be a certainty. Indeed, such degeneration must be marked — well progressed — for macroscopical evidence to be apparent. And even the mucosa of the fibroid body of the uterus may be carcinomatous without naked-eye appearance of it. Certainly, adenocarcinoma of the cervix may exist in the presence of uterine fibroids without attracting attention. Such cases have been reported by Lewis (3), Sutton (8), and many others, in which a second operation, a few days after removal of the uterine body, has been done to remove the cervix discovered to be the site of carcinoma, or when a study of the corporeal endometrium has proven it to be cancerous.

Certain it is that the careful inspection of the tumors and endometrium should be made as soon as the specimen is removed from the body. This may be done by an assistant, who may hold it convenient for careful survey by the surgeon before finishing the toilet of the peritoneum. This idea has received strong emphasis from surgeons, who, like Cullen (1), have had a continuation of a malignant process from an operation unnecessarily narrow as a result of failure to inspect carefully a specimen of uterine tumor at the time of removal. In some clinics the microscope is employed during operation. This must be of signal advantage as a routine procedure, and particularly so in special cases. It should not, in my judgment, act as a barrier against radical operation for growths clinically suspicious of malignancy. Nor should it be a substitute for thorough systematic examination in the laboratory.

Malignant degeneration of the cervix after removal of the uterine body no doubt occurs quite commonly, though Henrotin (2), in a discussion of this subject before the American Gynecological Society, lays particular stress upon the liability of clinically confounding granular tissue, from deficient blood-supply, with cancer of the cervix. He relates his experience in one such case, in which the pathological diagnosis of benign granular tissue had been clinically diagnosed as cancer and removal of the cervix performed. In the same discussion of Currier's timely paper, Newman exhibited a specimen and slides of a carcinoma that developed seven years after removal of the body of the uterus for multiple fibroids. The cancer was limited to the stump. C. P. Noble also referred to the 16 cases reported by Winter and to 2 of his own. Manton reported a case of sarcoma of the cervix discovered three months after removal of the body of the uterus for a large growth having the clinical appearance of a fibroid. Peraire and Bender (4) report a case of a woman of 52 years, in which they removed the body of the uterus for fibroids that microscopically contained no cancer. Four to six months after operation, cancer of the cervix developed. Richelot (6) reported three cases of this kind in thirteen cases of hysterectomy for fibroids.

The age of the patient and the family history as to cancer deserve consideration. As cancer of the uterus occurs most frequently after the thirty-fifth year, the advisability of removal of the cervix would seem to be greatest after that age. Sutton (8) found cancer of the body of the uterus, verified by microscopical examination, in 8 of 63 women beyond the fiftieth year that were operated on for uterine fibroids. Thirteen of them were over sixty years of age. If the family history shows that lineal ancestors have had cancer, an additional reason for removal of the cervix is thus furnished.

In years past the mortality of supravaginal amputation was less than that of hysterectomy. Technique, cleanliness, and experience in operating were incomparable to what they are now. Moreover, the complete operation was reserved for the severest cases — those complicated. Necessarily, the mortality rate was higher.

The vaginal route at that time had not been so amplified, and had not become so universal as at present. Naturally, the cervix is removed whenever the body of the uterus is removed by that route. Even suppurative adnexal disease furnishes an indication for vaginal panhysterectomy. So that removal of the cervix is considered of very little moment as regards modification of the mortality rate whenever removal of the body of the uterus is to be done, whether by the abdominal or the vaginal route. In my judgment, there is practically no difference in the mortality of the total and the subtotal operation. I am not aware of any reliable statistics of latter-day surgery to prove anything either way in this matter.

In the work of the tyro I readily admit that

by the abdominal route the amputation will be easier than the total extirpation. Even to the experienced pelvic surgeon, cases exist in which total extirpation will be a trifle more difficult. But is the surgeon to make the paramount issue his ease in operating? or shall the welfare of his patient be of first consideration?

I suggest that, as far as possible, the selection of the partial or the complete hysterectomy should be made before the endometrium or the interior of tumors is exposed. Otherwise, amputation preliminary to removal of cervix offers grave danger of infection of the peritoneum. The danger of such contamination is one of the fatal errors of myomectomy. If malignant disease be present in the tumor or endometrium, removal of the body and of the cervix separately unnecessarily exposes the peritoneum to contamination. Complete extirpation from above certainly increases the danger of injuries to bladder and rectum. Beyond this, no special danger exists in complete hysterectomy, as compared to the partial operation. Owing to bladder displacement in the complete operation, probably post-operation cystitis is more common. Surely, the vaginal support is lessened by it, unless the broad and round ligaments are sutured to the vaginal wall.

Having presented these facts, I am prompted to suggest that complete extirpation of the uterus is not an operation, even by the abdominal route, that necessarily taxes the patient's vitality to an extent appreciatively greater than that in the removal of the body alone, notwithstanding an impression has been inherited from the period of relatively bad surgery, when inexperience and bad technique combined to overwhelm the patient. By the vaginal route it is a much easier and more commonly practised operation than removal of the body and not the cervix. Injuries to the ureters and bladder now occur nearly entirely in the work of the incompetent surgeon. The objections to removal of the cervix with the body because of weakening of the vaginal roof and shortening the vagina are practically nullified by suturing the round and broad ligaments to the vaginal walls and by the fact that in employing the vaginal route these objections are forgotten. We have to consider as of great importance the fact that about five per cent of cases of uterine fibroids have malignant disease either complicating them or developing later in the stump, and also that in a considerable proportion of them the complication is not discovered until after operation. In those cases the patients have been endangered by the possibility of contamination of the peritoneum by severing the uterine tissue and canal and stimulation of the stump by traumatism, that may perhaps, in itself, produce a rapid extension of the malignant disease. Entire removal affords better drainage facilities, should they be required, and brings the pedicles within reach by the vaginal route, should their inspection or treatment be deemed necessary.

As against the removal of the cervix when the body of the uterus is removed, we have only the abdominal route to be considered. The comparative ease of the amputation is of moment only in critical cases. The cosmetic effect to young women deserves more consideration. Oftentimes, retention of the portio vaginalis is deemed by them to be of great value. It would seem, then, that in the young woman without a family history of malignant disease, and in the absence of uterine growths, the cervix should not be removed, if the body is to be removed by the abdominal route. In very critical cases in which the body of the uterus is to be removed by that route, even in the presence of tumors, it will occasionally be advisable not to remove the cervix. In practically all other cases the cervix should not be saved, and this injunction would seem to be the stronger in old women, particularly if evidence of uterine growths be present.

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COMMINUTED FRACTURE OF BOTH ENDS OF THE RADIUS

With Report of a Case

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THE subject of this report, a woman of 28, presented herself at my dispensary clinic, July 11, 1906, with the following history: On the 7th of July she fell down a short flight of steps and injured her right arm. She had no recollection of how she struck the ground, so that the mechanism of the injury was uncertain. She had been at once attended at her home by a local physician, who placed the arm on an internal angular splint and applied a tight bandage, which soon became so painful that on July 9th she applied at the accident ward of a nearby hospital, where the arm was examined and the dressings more comfortably applied. At a second visit, on the following day, she was informed that her wrist was broken and her elbow bruised. A straight splint was applied from the elbow to the metacarpo-phalangeal joints, and she was told to return in a few days.

When she appeared at my dispensary clinic on the 11th, an examination showed the following conditions: The entire right arm from axilla to finger-tips was greatly swollen and discolored, especially at the elbow and on the internal surface. Flexion and extension were limited to about twenty degrees on each side of the position of flexion at ninety degrees, in which the arm was placed; pronation and supination were markedly restricted; there was no crepitus about the elbow, and the measurements were normal, except the circumference, which was increased; pain was extreme on the slightest manipulation; Colles fracture was easily diagnosed. On account of the pain and swelling, no attempt at accurate diagnosis of the elbow injury, or of reduction of the Colles, was made, but the arm was loosely bandaged on an internal angular splint, continuous cold applications ordered, and the patient referred to Dr. W. Joline Martin for X-ray examination. The plates showed a comminuted fracture of the head of the radius and a comminuted Colles, but Dr. Martin did not consider them satisfactory, and a second set was taken (Figs. 1, 2), which showed plainly the transverse fracture of the head, the splitting of the fragment, and the displacement downward, inward, and forward. The vertical splitting of the lower fragment of the Colles is also plainly shown. The Colles had in the mean while been reduced and retained in anterior and posterior plaster splints, cut to avoid pressure on the ulnar styloid, after the method suggested by Pool and Lambert (1). The swelling at the elbow still persisted, in spite of the application of heat and astringent lotions, and the conditions of the plates could not be verified clinically. The treatment, therefore, was confined to attempts to reduce the swelling, and to slight passive motion every third day. Operation was considered, but was thought inadvisable on account of the condition of the soft parts.

The range of motion at first increased and the prognosis seemed favorable, but gradually the amount of passive motion the patient could tolerate decreased, especially in pronation and supination. At the end of the fifth week, therefore, the patient was informed that conservative treatment had failed, and that an operation for removal of the fragments was imperative if she wished to regain a useful arm. This was declined, and the patient did not return to the clinic, although asked to reconsider her decision, or, at least, to remain under observation.

The chief interest in this case centers in the fracture of the head of the radius, although the complication of a Colles fracture of the same bone makes it of greater rarity. A review of the literature of fractures of the radial head shows the condition to be rare and the literature itself scanty. Only fifty cases have been reported, and of these only two — those of Gordon (2) and of Verneuil (3) — were complicated by a Colles fracture of the same bone, and in both these cases the injury was of greater severity, that in Verneuil's case being sufficient to cause death. In the majority of the standard text-books the subject is dismissed with scant notice as being of rare occurrence. The excellent article of Thomas (4) has, however, placed the subject in a new light, and has called attention to the fact that these fractures are of much greater frequency than is usually supposed, and that many of the "sprained elbows" and injuries of uncertain character at the elbow may be fractures of the radial head. His citation of fifty-five unreported cases from the records of four skiagraphers of Philadelphia bears out this contention.

A study of the literature, of two personal cases which he reports, and of a series of experiments on the cadaver lead Thomas to conclude that the fracture is usually entirely intracapsular, often a simple splitting of the head, with little or no separation, and sometimes simply a vertical fissure or fissures. The mechanism is probably almost always the same; namely, the transmission of the force of the falling body, striking the ground with the arm extended,

Fig. 1. Wrist fifteen days after injury, showing comminution of lower fragment.

through the radius (which with its broad lower end receives practically all the force of a blow on the palmar sufrace of the extended hand), usually resulting in a Colles fracture, if in any injury, to the radial head and thence to the shoulder. Van Lennep (5) says that this mechanism results in injuries occurring in order of frequency from below; e. g., Colles fracture of the radial head, fracture of the lower end of the humerus (usually the capitellum), and injury at the shoulder-joint; and, in the light of Thomas's experiments, this would certainly seem to be the case, though the majority of the injuries to the head of the radius have probably been overlooked.

The position of the fragments depends largely on the integrity of the orbicular ligament, which, as it surrounds the radial head, holds the split ends of the bone in apposition. As the force is transmitted *through* the encircling ligament, it can readily be seen how a severe injury is necessary to rupture the ligament by forcing the fragments apart. In my own case I believe the ligament to have been torn. A careful inquiry into family and personal history failed to elicit any condition which could account for the extent of the injury, for the fall was short and the patient not at all heavy; and in his experiments on the cadaver, Thomas found that considerable force was necessary to produce even a fissure. It is to be regretted that the patient's refusal to undergo operation resulted both in her own disability, which will doubtless be permanent, and in our inability to determine more accurately and positively the position of the fragments and the condition of the bone and of the orbicular ligament.

As to the treatment, the literature contains very little of conclusive value, the various authors differing widely as to the advisability of operative treatment, usually basing their opinion on a few personal cases alone. Five of Stimson's cases (reported by Thomas)

Fig. 2. Lateral view of elbow twenty-four days after injury, showing comminution of displaced head of radius.

required operation. Thomas's two cases did not, and they recovered good function, and he concludes that "excision of the head under any circumstances, even excision of the detatched fragment, will rarely be required." At the same time he qualifies this by stating, under the head of treatment, "If the fragments were widely separated, operation would probably be necessary for the removal of the fragment or of the whole head." In my own case I believe the injury to have been of sufficient severity to demand operation, and that the question to be decided was the time of operation. As to the time of operation, if indicated, while one case hardly justifies the expression of an opinion, it seems to me that in the presence of extensive œdema it is wise to avoid opening the joint, if possible. Cheyne and Burghard (6) advise immediate operation. Four of Stimson's operative cases were operated on from three to eight weeks after injury, and in one the time was not mentioned. The lack of detail in the previously reported cases, together with the contrast between the good functional results obtained in cases of moderate injury by Thomas, and the bad result in my case of extensive injury, would suggest the impossibility of making any fixed rule as to operation.

In case operation is not thought advisable or is refused, the question of treatment centers on the time and the amount of motion employed and allowed. Thomas is inclined to aim first at obtaining union of the fragments, and motion of the joint afterward. It must be remembered, however, that many cases improperly diagnosed as bruises or sprains have recovered good function under early and free motion. This question also remains to be settled by greater details given in reported cases.

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FOREIGN BODIES WITHIN THE VAGINA[1]

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IT is rather remarkable that in the literature of foreign bodies within the female generative organs the English and American contributions are conspicuously scant. For this no adequate cause may be assigned, for it would be somewhat casuistic to assume that the Anglo-American female is relatively exempt from this condition; nor, on the other hand, may we lightly dismiss the subject as one devoid of genuine scientific import. The case to be hereinafter detailed is interesting, though not unique.

VARIETY OF OBJECTS FOUND — CASES CITED

Objects of the most diverse character have been found within the uterovaginal tube. As might be anticipated, the variety found within the vagina is the more diverse. Among a long list, the following are probably the most frequently encountered: Goblets, bottles, hairpins, needles, pencils, balls of various sorts, beer-glasses, compasses, bobbins, pessaries, etc. Considerable variation in duration of lodgment, also pathologic changes induced, are to be noted.

Hubbauer (1) records a girl of 19, in whom a glass was found in the vagina, fixed, and held solidly by incrustations. Within the vagina for six months. Removed with difficulty. Holmes (1) notes the neck of a bottle within the vagina, a point of which had penetrated into the bladder. A calculus had formed on both ends. Cloquet (1) mentions the autopsy notes on a woman who had for a long period carried a pewter goblet within her vagina. Oxide of lead was found in the gangrenous *débris*. In *L'Union Medicale*, Paris, March 4, 1848 (1), is recorded the case of a girl "addicted to onanism." She died from the effect of a pewter cup which she had inserted in her vagina. This cup had been in place fourteen months. Shame led to concealment. Examinations had always been refused. It is of record (1) that a country girl consulted Dupuytren at Hôtel Dieu, stating that several years before she had been violated by some soldiers, who had introduced a foreign body within her vagina, which she had never succeeded in removing. Dupuytren found a small metallic pot two inches in diameter, concavity toward the uterus, and containing a solid black substance with a most fœtid odor.

PROLONGED RESIDENCE — CASES CITED

The tolerance of the vaginal tissues to the presence of a foreign body, as evidenced by the extended periods of time during which the body is harbored, and the frequently insignificant tissue change that is produced, is of both interest and import.

Breisky (1) reports a case of dysmenorrhœa for which a cotton-reel was inserted seven years previous, with good recovery.

Pfeifer (2) records a fistula rectovestibularis produced by a Zwanck-Schilling pessary which had been applied for a retroflexion nine years before. During the last six years it had not been disturbed. Three years ago, patient noticed fæces escaping from the vagina.

Bazzanella (1) removed a drinking-glass from the vagina, which had been inserted ten years before by the husband of the patient.

Peters (3) reports a Mayer's soft rubber pessary nine centimeters in diameter that had remained within the vagina for ten years without douche or removal, in a woman of sixty.

Nagel (4) removed, manually, and without difficulty, a Hodge pessary that had remained continuously within the vagina for eleven years. A terrific odor was encountered, and a deep granulating furrow, which, however, soon healed.

Pearse (1) speaks of a woman of 36 who had a menorrhagia for ten days, with severe colicky pains and great prostration. A silk bobbin was found "one inch from entrance," which had been introduced fourteen years before. This woman had had attacks of peritonitis, hemorrhages, etc., and a urethrovaginal fistula was found. An interesting fact appears in that she had been twice married and ex-

[1] Read before the Medical Society of City Hospital Alumni, October 4, 1906.

amined by several physicians during this period, but this three-fourths-inch-long bobbin had never been discovered.

Jungengel (5) records a large fæcal fistula caused by presence of a Zwanck pessary for fourteen years, within the vagina. Cured by operation.

Marshall (6) speaks of a woman of sixty who sought relief from urinary disturbance, intense lower abdominal pain, and bloody discharge of several months' duration. Examination disclosed an extensive ulceration of both anterior and posterior vaginal walls; with an induration, resembling granulation tissue, within the vagina. A diagnosis of a malignant neoplasm was made. In reality, however, the ulceration was produced by a pessary which was found to have remained within the vagina for fifteen years. The mucosa had completely bridged over. No malignancy existed.

Pinet (1) mentions a pessary carried for twenty-five years.

Szigethy (1) saw a woman of 75 years, that, thirty years previous, had introduced within her vagina a ball of string that had been dipped in wax, for the relief of a prolapsed uterus. This ball, seven inches in circumference, was worn without discomfort during these thirty years, and was finally expelled while undergoing some violent effort. It was covered with mucus, but otherwise unchanged.

In *Russky Vrach* (7) is related the case of a woman, who, to support her uterus, introduced into her vagina a croquet-ball. The improvement obtained by this support was so great that she left it in place for thirty years! Finally, on account of the irritation it produced, it became necessary to remove the ball; the extraction proved difficult.[1]

Füth (8) mentions a widow of 64, that stated a ring had been inserted by a midwife thirty years before. It had not since been removed nor otherwise attended to. For a number of years an offensive discharge existed, which finally became unbearable, "worse even than necrotic carcinoma." Ulceration of the vaginal wall might have been anticipated, but nothing like this was found, for, after the pessary, a wooden ball, had been removed piecemeal, the mucous membrane was found merely slightly reddened.

Alexander (9) has reported an incrusted pessary retained for 32 years.

Jackson (1) reports a "glove pessary" that remained in the vagina 35 years.

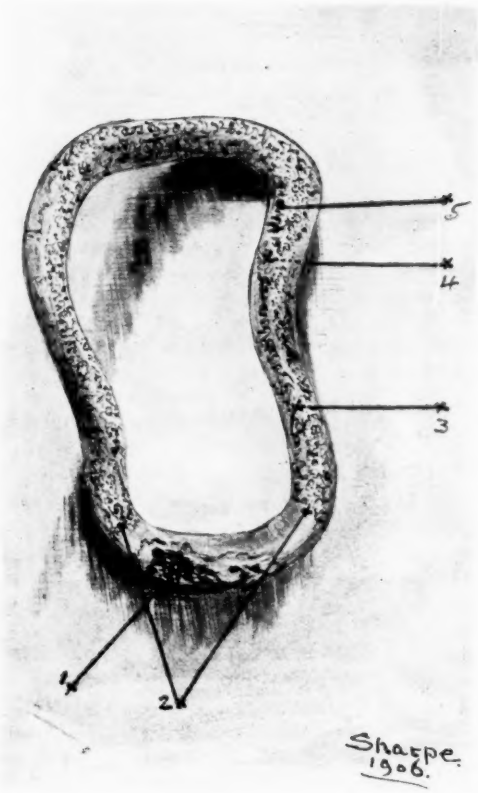
Gosselin (1) reports a pessary incrustated with calcareous salts that was found 36 years after insertion.

Maly (10) alludes to a working-woman of 67 years who menstruated at 20, menopause at 45. One confinement forty years ago, with a lacerated perineum. First symptoms of a beginning prolapse nine months later, for which a soft rubber ring was introduced by a physician. This was worn for fully 39 years. For financial reasons, patient personally removed and cleansed pessary. To facilitate removal, she inserted it in the lower vagina. All went well up to within eight months, after which moderate hemorrhages, purulent discharge, and slight pains supervened. During the last ten weeks the pains and discharge increased, the latter becoming offensive. During this period cleansing was more frequent. Symptoms continued to increase, which, together with œdema of the external genitals, brought the patient to the hospital. Examination revealed a deep circular furrow, which extended obliquely upward from the urethral swelling to the mid-posterior wall. This sulcus lay within an extensive indurated ulceration, which occupied more or less of the entire lower half of the vagina, including the urethral swelling and orifice, as well as part of the vulva. Upper half of vagina and internal genitals perfectly normal. Microscopic examination confirmed diagnosis of carcinoma.

Hurxthal (1) records the removal of a pessary carried for 41 years.

Gerould (1) mentions a German woman of 84, whose vagina inclosed a pessary for more than fifty years. Its presence had been forgotten until reminded of the fact, some few

[1] It would be a matter of considerable interest if information was accessible as to the size of this ball. Unfortunately, it has proved impossible to consult the original, or even to amplify the notes as they here stand. Inquiry in the shops show that the regulation croquet-ball in the United States is 9 cm. in diameter by 28 cm. in circumference — unquestionably a large mass to be retained within the vagina for thirty years, and speaking strongly for no inconsiderable relaxation of the vaginal tube. On the other hand, it is remarkable that the weight of the ball did not suffice to cause its descent and release; in other words, that it would not only not act as an efficient pessary, but would automatically cause its own exit. Yet this ball may not have been of regulation size; it may have been a juvenile ball, or one of any number of intervening sizes.



Hard rubber Hodge pessary, retained in the vagina 35 years, during which time it remained undisturbed 25 years.
1. Calcareous incrustations (slight).
2. Inclosing only non-eroded area.
3. Most deeply eroded area (posteriorly).
4. Calcareous incrustations (posteriorly).
5. Calcareous incrustations (insignificant).
Structure diameter, 8 mm.
Extreme transverse spread, 5 cm.
Extreme longitudinal spread, 7.75 cm.

years before death, by a local irritation. This pessary was found, it is claimed, to have largely retained its original wax covering.

Mackey (1) has removed a glass pessary which remained within the vagina 55 years.

A sufficient number of cases illustrative of variety of character and period of habitation have here been given. Extensive records are accessible to those who may be particularly interested.

The critical mind may be somewhat skeptical in unreservedly accepting these remarkable histories; yet it may not be gainsaid that the vagina is often phenomenally tolerant over extended periods of time and under adverse conditions that ordinarily favor pathologic change.

PERSONAL CASE RECORDED

The following personal case is placed on record:

Mrs. A. B., widow, aged 75 years, was seen January 12, 1903. She states that she has in her vagina a pessary that has been there for 35 years. During the latter 25 years it has not been removed. Patient's knowledge of original condition for which pessary was inserted is so vague as to be unreliable. Examination shows rather extensive adhesive overbridging of the vaginal tissues, laterally, anteriorly, and posteriorly. The pessary was completely enveloped, save for a small area anteriorly contiguous to the pubis. A chronic vaginitis existed; the vagina had become distorted into an hour-glass or funnel shape. Measurements and accurate palpation proved impossible. Suitable douchings were made until January 29th, when the pessary was removed. With moderate difficulty it was released from the guttered and misshapen vagina, and the latter cleansed and appropriately packed. Packings were removed on eleventh day, and sterile saline solution douches permitted. November 30th, ten months later: Good result from operation. No evidence of malignancy present. Vaginal deformity still present, but considerably less than at date of operation, and causing the patient no inconvenience. To-day, over three and a half years later, I am assured by her attending physician, Dr. Walter Baumgarten, that the patient is in excellent general health, and that no evidence of malignancy is present. The pessary — hard rubber Hodge — measures 5 cm. extreme transverse spread by 7.75 cm. extreme longitudinal spread; structural diameter, 8 mm. Examination shows pubic curve, with almost unimpaired polish; the polish of nearly the remainder of pessary has disappeared under erosion. This erosion is, with the exception of an area on left bar posteriorly and right bar anteriorly, not marked. Slight incrustations found opposite urethra, also small areas on left bar, both anteriorly and posteriorly. Neither color changes nor odor of special interest; rubber not friable nor honeycombed. It is remarkably interesting that a 35-year residence has produced such insignificant structural disintegrations. It may be added that operation was not sought by the patient, for the pessary caused her practically no annoyance. Its removal was advised upon the ground of safeguarding the patient's welfare, and accepted on that basis.

TOLERANCE OF THE VAGINA AND INJURIES SUSTAINED

The literature shows that not only is the vagina tolerant, ordinarily, to glass and rubber

bodies, but, in turn, exerts remarkably insignificant destructive changes upon these substances. Metals, such as those found in the vagina (with the possible exception of gold and silver, which are not commonly encountered), do not, however, follow this rule, for not only are they considerably disintegrated, but the vaginal tissues are subjected to the risk of serious involvement.

Cloquet (1) cites a case where the cork core of a pessary was found rotted within the vagina. Sponge is apparently an unwelcome guest, and yet so unpromising a mass as a ball of string dipped in wax, as recorded by Szigethy (11), remained for thirty years within the vagina, and on removal, save a coating of mucus, was unchanged; the patient herself having at no time suffered ill effects.

The literature apparently shows a marked resistance on the part of the lateral vaginal walls to serious involvement or perforation, and an integrity of the vesicovaginal septum considerably more resistant to these casualties than the rectovaginal. It may be held that this condition is due to the configuration of the pessary, rather than to any difference in resistance to be found in the vesicovaginal and rectovaginal structures. This point is based, however, not exclusively on pessary cases, but seems to obtain when consideration is made of foreign bodies of all sorts imprisoned within the vaginal tube. The case of Hubbauer (1) to which allusion has been made, is a fair sample of vesicovaginal perforation. The following exemplifies rectovaginal involvement in a minor degree: Hildebrandt (12) found in a woman, 71 years of age, with a large genital prolapse, that a pessary had been inserted. Within five months the vaginal wall became eroded, and the club-shaped end of the pessary pointed in the rectovaginal septum, like a collar-button. The wall resisted perforation. Pfeifer's (13) case, noted above, illustrates an instance of complete perforation of rectovaginal septum.

In this connection, Neugebauer's (16) findings are of interest. He reports from the literature: Perforation of rectum, 23 cases; of bladder, 20 cases; of bladder and rectum, 10 cases; uretero-vesicovaginal fistula, 1 case; ureterovaginal fistula, 1 case; urethral perforation, 1 case; cul-de-sac perforation, 2 cases; penetration into pelvic connective tissue, 3 cases (in one of which the rectum was also penetrated); perforation of uterus, 6 cases.

DIAGNOSIS

That the diagnosis of a foreign body within the vagina is not always devoid of difficulty is evidenced by the case of Pearse (14), where a silk-bobbin remained undiscovered in the vagina for fourteen years, and this, too, in spite of the fact that the patient had been twice married and subjected to the examination of various physicians.

The following case of Guyon (1) is of interest. He reports a patient suffering from continuous vaginal hemorrhage. She had had extensive treatment, but to no avail. Finally, when patient had reached the stage of extreme exhaustion, "injections of vinegar-water" solved the difficulty, for she was delivered of a live leech (of a species common to the country). The hemorrhages ceased and health was restored.

Poulet (1) reports a case of a pregnant woman who had been examined by a physician. He made a diagnosis of a carcinomatous degeneration of the cervix. Capuron, who saw the case in consultation, failed to confirm this diagnosis. Further examination revealed a sponge that at some previous time had been introduced.

The case of Marshall (15), abstracted above, where malignant neoplasm of the vagina was suspected, but a fifteen-year, completely enveloped pessary was found, and no malignancy, is also apposite.

STRUCTURAL AND CONFIGURATIVE CHANGES

The diversity of structural change in the vaginal tissues is marked — exemplified at one extreme by case of Füth (17), where, after thirty years, the mucous membrane was found but slightly reddened, though both foul discharge and foul odor were in evidence, and at the other extreme by the cancer group, to which allusion will be made later.

Ordinarily, we may expect to find more or less marked inflammatory changes in the vaginal mucosa. A severer grade will exhibit changes of submucosal structures, while in what might be termed the third degree we would

class abscesses, gangrenes, and penetration of hollow viscera.

It may be made as a broad statement, to which, however, an occasional exception will doubtless be found, that most, if not actually all, of the detrimental sequelæ following pessary occupancy of the vagina are due to uncleanliness. A vaginitis begins, at first not more than a mucosal blush, which may extend to a severe degree; ulcerations, at first superficial, later deep, follow, either single or multiple. The connective tissue participates, until, if pressure and uncleanliness be permitted to continue, pus, abscess formation, and gangrene may, one or all, be established. After a certain period, which, as has been demonstrated, varies widely, the pessary begins to imbed itself; it forms its gutter partly by direct pressure, partly by muscular contractions of contiguous structures, partly by intra-abdomino-pelvic pressure, and partly by proliferation of the pavement epithelium surrounding it. This proliferation may, aided by forces just suggested, finally completely overbridge the pessary, and organization follow until the bridge closely resembles the vaginal walls. (Such prevailed in the case just recorded.) Veit shows that true granulation tissue is ordinarily found only at the edges circumjacent to the gutter. These occasionally closely simulate carcinomatous degeneration. Bridges the thickness of the little finger have been encountered. Prompt recovery usually follows removal.

The vagina, as a whole, is frequently distorted; distended above and contracted below the pessary, producing an hour-glass or funnel shape. The proliferation at the edge of the ulcerated furrow, constituting a border line, may assume almost the consistency of a cicatrix. If, now, this wall be long, orientation becomes difficult; at times impossible. Neugebauer records as complicating conditions, one case of stenosis of os with pyometra (death), one case of ileus, and several cases of septic peritonitis. Revillet notes the same.

The case reported by Naef (21) introduces yet another complication, which may considerably augment the difficulty of removal. A woman, 72 years of age, with pronounced prolapse, had introduced a hard rubber pessary, with good results. One year later, its removal could be accomplished, but with the greatest difficulty, and then only by piecemeal delivery, for the introitus vaginæ and vulva had become stenotic. (The question might be here raised as to whether this "stenosis" might not have been due to ordinary senile changes.)

MALIGNANCY — CASES CITED

But, far outweighing all other matters of interest is the possibility of a foreign body long resident within the vagina producing or assisting in the production of malignant degeneration of contiguous structures. The vagina is not one of the choice sites for the development of primary cancer. Gurlt (18), from a material of 59,600 patients, observed primary cancer of the vagina in 114 instances (.19 per cent); while Schwarz, in 35,807 patients, found 84 cases of primary cancer of the vagina (.24 per cent). On no other ground are we able adequately to account for the remarkable relative innocuity of the long resident pessary within the vagina. Neugebauer (19) reports eight cases of carcinoma due to pressure and irritation. Maly's (20) case has been detailed above.

Six pertinent cases may be found recorded by Veit (22). They are, in brief, as follows:

Hegar. Woman of 56. Retroversion and cystocele. Hodge pessary employed. Cancer at anterior vaginal wall at point of pressure over os pubis.

Kaltenbach. Cancer of posterior vaginal vault, corresponding to pressure from posterior bar of a Hodge pessary. (Not unimpeachable, however.)

Meyer. Woman of 60. Prolapse. Soft Mayer's ring used; later a celluloid Hodge. Two and one half years later, after profuse bleedings and suspicious "erosions" had been twice observed, a papillary cancer was found involving the posterior vault. Confirmed microscopically.

Schmidt. Cancer of the vagina, following use of Mayer's ring for fifteen months, without cleansing.

Von Winckel. Cancer of the vagina, caused by Zwanck's pessary.

Schwarz. Woman of 75. Cancer of the posterior vaginal wall, produced by completely incrusted pessary. Within the vagina twelve years without cleansing.

CONCLUSION

Fortunately for the careless patient and the careless physician, the uncleanly patient and the non-observant physician, though disastrous by-effects may be developed by ill-fitting and uncleanly pessaries, and by the pessary not adequately controlled, yet cancer does *not frequently* develop. That *it does occur* is sufficient to keep the careful man constantly on guard, and is ample warrant, of itself, in addition to numerous other sound surgical reasons, for the prompt removal of any and every pessary that has ceased to be a therapeutic agent and become a foreign body.

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OCCLUSION OF THE FEMORAL VEIN

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SUDDEN occlusion of the femoral vein, at or near Poupart's ligament, is followed by such markedly different consequences that it is impossible to determine the seriousness of this occurrence, or to predict with any degree of positiveness the result when a femoral vein is ligated at the base of the thigh. The clinical observations of Lidell (1), Roux (2), Pilcher (3), Braun (4), and others show that ligation of the femoral vein at the root of the limb is a very serious and uncertain procedure indeed. More uncertain as to its consequences, perhaps, than any other major operation, and more serious than the ligation of any other accessible vein, ligation of the large veins of the neck is, according to Sappey (5), infinitely more safe than ligation of the femoral vein. Nicaise (6) makes a similar statement regarding the large veins of the upper extremity; while the experience of Marconi (7), Bottini (8), and Hartmann (9) seems to show that ligation of the vena cava is much less serious than ligation of the femoral vein at Poupart's ligament. These men have "repeatedly ligated" the vena cava in removing renal tumors, "and in no case has harm resulted" — quite a contrast when compared with the results following ligation of the femoral vein. Braun (10), after carefully reviewing clinical cases, and performing forty experiments on cadavers, states that gangrene of the extremity may be expected to occur in forty per cent of the cases of ligation of the femoral vein at or close to Poupart's ligament.

The possible results of occlusion of the femoral vein, and the question of how to obviate them, are factors of practical importance in operating upon malignant growths in the groin. Such a case came under my care about seven months ago. This patient, a man 26 years of

Photograph of patient.

age, had a sarcoma in the left inguinal region that was first discovered accidentally one year previously. The growth was rapidly increasing in size, and had produced some œdema of the leg for three months. It extended from a point three inches below Poupart's ligament into the pelvis; was firmly fixed, and seemed to be more intimately associated with the femoral vein than with the artery.

In planning the operation for this case, it was thought that a resection of the femoral vein, which seemed to be indicated, would not be followed by serious consequences. The prolonged pressure of the tumor on the vein would result in the formation of a competent collateral venous circulation. Thirteen cases have been recorded (11) in which the femoral vein has been ligated in removing inguinal tumors, and in none of these cases did gangrene ensue.

In performing the operation in the case herewith reported, it was found that the femoral artery could be saved, but the internal saphenous vein, the large muscular tributaries of the femoral vein, and all of the venous branches from the termination of the deep femoral vein to the beginning of the common iliac would have to be divided to remove the tumor thoroughly. The removal of these venous branches destroyed the possibility of the formation of an anterior collateral venous circulation; and if the femoral and external iliac veins were resected, there would be practically nothing but the obturator and ischiatic veins left to return the blood from the extremity. It was decided to dissect the tumor from the vein and leave the vessel intact, if possible; and if this could not be done, to resect the vein and, according to the advise of Pilcher (12), ligate the superficial femoral artery. It was possible to dissect the tumor from the vein without wounding it.

The dangers in this method were anæmic necrosis of the vein resulting in hemorrhage, and the leaving of a part of the sarcoma in the venous walls. No hemorrhage occurred, and at this time, six months after the operation, there is no evidence of recurrence of the tumor. It was hoped that by leaving the vein immediate gangrene would be prevented, and that by the time the vessel became occluded by thrombosis an efficient collateral circulation would be established. After the operation the limb remained normal for eight days, then it began to swell, and has remained swollen ever since. At this time, six months after the operation, the entire extremity is considerably larger than its fellow (as shown by the photograph), and does not seem to be diminishing in size. It is firm, but not cyanotic or œdematous. It is not painful. A silk stocking is worn. For three months the patient has worked constantly — doing the work of an ordinary farmer.

Ligation of the femoral vein for acute conditions may or may not be followed by gangrene of the limb. The results are not constant. It is probable that the anastomotic connections between the veins of the lower limbs and those of the trunk vary greatly in different subjects. This contention is supported by the opposite conclusions come to by those who have investigated this subject experimentally on cadavers. Richet (13) and Verneuil (14) claim that the anastomotic connections between the thigh and trunk are very few. Braune (15), as a result of his anatomic and literary investigations, says that usually the femoral vein has no collaterals to re-establish the circulation when ligation has been done; while Nicaise (16) states very positively that "after ligation of the femoral vein at the root of the limb, the return circulation is abundantly assured." This statement, however, is based on a review of Sappey's work and the performance of a single experiment.

The experiments of Braun (17) of Heidelberg are sufficiently numerous to be accepted as giving reliable information regarding the blood-paths collateral to the femoral vein. Braun performed forty experiments. In forty per cent of these it required a pressure of more than 150 mm. of mercury to pass fluid from the limb to the trunk after the femoral vein had been ligated. He concluded that gangrene would follow in forty per cent of the cases after ligation of the femoral vein in acute conditions, and that in one third of the remaining cases — that is, twenty per cent — the results would be doubtful.

Clinical experience seems to prove the correctness of Braun's observations. Of five cases of ligation of the femoral vein for acute conditions, two died of infection, one recovered, and in two gangrene occurred. Sands (18) reports another case in which gangrene occurred, and Weir (19) reports a similar case. During the Civil War there occurred two cases of wounds of the femoral vein, in which it is probable — although the reports are very incomplete — that gangrene resulted.

Conditions are quite different when the femoral vein is occluded slowly by thrombosis, or when ligation is done after prolonged compression of the vein. In these instances the gradual occlusion of the vessel permits of the formation of an efficient collateral venous circulation. This, in all probability, is what occurred in the case herewith reported. Pilcher reports a similar case. There seems to be little danger of gangrene resulting from ligation of the femoral vein if prolonged previous compression of the vein has existed. Braun reports thirteen cases in which the vein was ligated during the removal of inguinal tumors and no unpleasant results followed. Under certain conditions, however, ligation of the femoral vein, during the removal of inguinal tumors, becomes equally as serious as occlusion of the vein made necessary by acute conditions. Such is the case when, in order to remove the tumor, it is essential not only to ligate the vein, but to destroy its collateral branches. In fact, ligation, under these circumstances, is more likely to be followed by gangrene, than is simple ligation to control hemorrhage.

Of the various methods that might be suggested to prevent gangrene after ligation of the femoral vein, none can be relied upon. The interposition of a neighboring venous segment, as suggested by the work of Carrell and others, might be utilized to advantage in dealing with some of these cases. The advice of Lidell (21) to ligate the common femoral artery in cases of wounds of the femoral vein was based upon three cases — those of Langenbeck, Roux, and Linhart. This advice, in the light of more recent clinical and experimental evidence, seems unwarranted. In eleven cases collected by Küster (22), Braun, Pilcher, and others, in which ligation of the common femoral artery was resorted to in treating injuries of the femoral vein, gangrene resulted in six of them.

Pilcher has recommended that the superficial femoral artery be ligated to prevent gangrene after ligature of the femoral vein for acute conditions. The value of this procedure is not known. In Pilcher's case, he was not positive that he had not ligated the common femoral instead of the superficial femoral artery; and he was not sure that he had not ligated the femoral vein below the termination of the internal saphenous.

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THE CŒLIAC AND MESENTERIC PLEXUSES AND THEIR ROLE IN ABDOMINAL SHOCK

An Experimental Study

From the Breslau Surgical Clinic—Professor Garré

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THE well-known experiments of Goltz, who showed that it was possible to produce stoppage of the frog's heart by mechanical irritation of the abdominal viscera, laid the experimental foundation for the theory that reflex action is accountable for abdominal shock. We use the latter term to designate that characteristic symptom-complex which may follow trauma of the abdomen and attend certain diseased conditions of the peritoneum, which may occur after abdominal operations or at the very onset of many pathological processes in the peritoneal cavity.

The inability of later experiments to imitate the phenomena observed in the frog, in the case of the warm-blooded animals, has caused many to look elsewhere for light upon this subject. The advances in bacteriology and in our knowledge of the toxins in the past two decades have directed nearly all attention to these lines of thought. Thus it is that most of the later theories have had recourse rather to the bacteria and the toxins in their explanation of the collapse and death in peritonitis, and that the reflex theory and the role of the abdominal sympathetic have come to play a subsidiary part.

It seemed wise at this juncture to attack this problem from the neglected aspect. The questions to be considered were:

1. *What is the role, if any, of the abdominal sympathetic in abdominal shock?* and

2. *Can a condition of collapse or shock be produced in warm-blooded animals by irritation of the abdominal sympathetic ganglia?*

The negative results obtained in the study of the second question led to the consideration of the following:

3. *Under what circumstances, if any, is it possible to cause a condition of collapse or shock by irritation of the abdominal sympathetic ganglia?*

After experimental evidence had been obtained showing that impulses passing through the abdominal sympathetic could, in the frog, produce a condition of shock, a number of investigators undertook the study of the large abdominal ganglia. It was found that these were not essential to life in the animals studied (dog, rabbit, cat); that they could be destroyed, injured, or extirpated without causing marked physiological disturbance. A great number of experiments of this sort was done; but the results were, in general, unconvincing; a large proportion of them, done in preaseptic days, being marred by the occurrence of complicating peritonitis. Even to-day, however, our knowledge as to the functions of the abdominal sympathetic ganglia is still meager, and the results of extirpation are by no means uniform.

These considerations led us to begin our research with:

I. A Study of the Effect of Extirpation of the Cœliac and Mesenteric Ganglia

Inasmuch as the literature of this subject has been exhaustively and carefully reviewed of late by Strehl, we shall confine ourselves here to a short *résumé* of the more important results that have been obtained.

In the oldest experiments (those of Pincus, Samuel, and Budge), the investigators were unable to keep the animals alive much longer than twenty-four hours. Their observations may therefore be left out of consideration. The more important conclusions reached by the other experimenters may be summed up as follows:

(*a*) Animals may live for days and months after the extirpation of the cœliac ganglia. Lamanski was able to keep one dog, out of twelve operated on, alive. Of the fifteen rabbits experimented on by Peiper, eleven survived the operation. Lewin and Boer performed total extirpations of the ganglia in seventeen rabbits, seven of which lived from

fourteen to thirty days; but concluded that the ganglia are essential to life. Popielski succeeded in keeping dogs alive for fourteen months after extirpation. Of the twenty-two cats operated on by Strehl, eighteen survived longer than two days; and of these, six were killed between the sixth and thirty-fifth day. In the most recent piece of work, Pieri reports a series of eight successful extirpations of the cœliac ganglia in rabbits.[1] The animals which were killed about two or three months after operation remained perfectly healthy till death, gained in weight, and showed no lesions in the organs at autopsy.

(*b*) Disturbances of the intestinal tract may follow extirpation, as is manifested by the occurrence of temporary or persistent diarrhœa. Lewin and Boer consider the paresis of the intestines, complicated with diarrhœa and meteorism, an important feature. Popielski observed diarrhœal and bloody fæces shortly after operation. Strehl also observed diarrhœa between the second and tenth day. Others (Adrian, Lustig, Peiper) report no such disturbance.

(*c*) Temporary glycosuria, with or without acetonuria, may be observed after extirpation. Munk and Kleks report the occurrence of diabetes mellitus after partial extirpation of the cœliac ganglia. Lustig found that this was but temporary, never lasting more than two days; acetone, however, was regularly present. Peiper, on the other hand, was never able to detect acetone, and mellituria was present for but a short time in five out of fifteen rabbits observed.

(*d*) Other observations. Most of these deserve scant consideration, inasmuch as they are either chance occurrences or the results of complications, particularly peritonitis. We may mention however, ulcers of the stomach, duodenum, and small intestines.

From this brief review of the literature it is readily seen that conflicting results have been obtained by previous observers, and it seemed wise to repeat the experiments in dogs. These animals were chosen because the least satisfactory work had been done upon them, and because, through their greater powers of resistance, they seemed more suitable for the extensive abdominal operations that experimental production of collapse might require. The correctness of this assumption was further evidenced by a number of preliminary extirpations done on rabbits, the first two of which died shortly after operation. It was necessary to use animals which could be counted on to survive the operative procedure.

The anatomical *data* which we were able to find in the literature bearing upon the ganglia in the dog were scattered and insufficient. We have therefore decided to give the results of our anatomical studies.

[1] It is not made perfectly clear in this article just how completely the extirpation was done.

ANATOMY

The following nerves supplying the abdominal contents and peritoneum will be discussed: 1. The Sympathetic; 2. The Cerebrospinal Nerves: The Vagus; The Phrenic.

1. The Sympathetic

A study of the distribution of the sympathetic nerves in the abdomen of the dog shows that the major portion of the abdominal contents receives its supply through the cœliac and mesenteric plexuses. To the cœliac plexus belong two large ganglia (the ganglia cœliaca); to the mesenteric plexus belongs the ganglian mesentericum superius. Attention was particularly paid to these three ganglia; for it is through them that we can attack the greater part of the territory of the abdominal sympathetic system. In the following anatomical discussion we will consider: (*a*) The site and relations of these three ganglia; (*b*) Their connections and the distribution of their branches; and (*c*) Their connections with the central nervous system.

(*a*) *The Left Cœliac Ganglion.* This is an elongated, flattened, ganglionic structure, situated above and slightly to the inner side of the upper border of the left adrenal, either directly to the left of the cœliac artery or between it and the superior mesenteric artery. It can be distinctly seen shining through the peritoneum, and appears to be retroperitoneal when the intestines with their mesentery are pulled over to the right. It lies, however, in the root of the mesentery. Its distance from the adrenal varies slightly, and its lower extremity, when it connects with the superior mesenteric ganglion by a short, ganglionic strand, comes into contact with the upper pole of that organ. Its relation to the large lymph ducts is of importance. These structures lie retroperitoneally, and in the root of the mesentery, just above and somewhat median to the upper pole of the left adrenal, often very close to the median border of the ganglion.

The Right Cœliac Ganglion. This ganglion is a larger, somewhat broader structure, lying in the root of the mesentery, close to the origin of the hepatic artery, in contact with both it and the right border

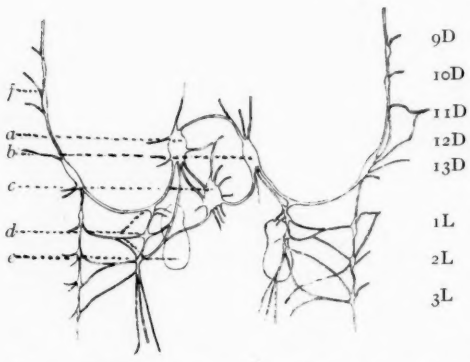

Fig. 1. Showing the connections of the right and left cœliac ganglia of the dog, with the gangliated cords. Diagrammatic. The adrenals are dislocated to the midline, and the view is from the posterior.

a. Right cœliac ganglion.
b. Left cœliac ganglion.
c. Mesenteric ganglion.
d. Adrenal plexus.
e. Adrenal (right).
f. Gangliated cord.

of the cœliac axis. If the mesentery of the duodenum and the duodenum be pulled to the left, this ganglion will be seen shimmering through the tense peritoneum, just to the left of vena cava and at the root of the hepatic artery. The best guide to the ganglion is the root of that free fold of peritoneum, the ligamentum hepatogastro-duodenale, which passes from the hilus of the liver downward, curving backward to blend with the peritoneum over the posterior abdominal wall a short distance to the left of the vena cava. It bounds the foramen Winslowi, and contains the vena portarum and the arteria hepatica. The cœliac ganglia of the two sides are separated only by the cellular tissue of the mesentery, the lymph-vessels, and the cœliac artery, and are covered by the respective peritoneal layers of the mesentery.

The Superior Mesenteric Ganglion. This ganglion lies from 0.5 to 1.5 cm. below the right cœliac ganglion, in the root of the mesentery, in close contact with the superior mesenteric artery. It varies considerably in form and position, inasmuch as it may be connected with the left cœliac by a very short strand, or may lie at a considerable distance from it. It is found just to the left of the vena cava, in that prominent fold of the mesentery formed by the superior mesenteric artery. It often lies wholy cephalad to this vessel, but sometimes curves around its caudal border.

(*b*) These three ganglia are very intimately associated with one another by various nerve fibers distributed through the mesentery. Sometimes the left cœliac and the superior mesenteric ganglia are fused at their lower poles so as to form a crescentic ganglionic mass about the superior mesenteric artery. The connection between the right cœliac and superior mesenteric ganglion on the one hand, and the right cœliac and the left cœliac ganglia on the other, may be quite as intimate; but usually the connecting branches are longer and finer. Although the three ganglia are usually quite discrete, they may be fused so as to form a ring-like ganglionic mass.

These ganglia, through their branches, supply the greater part of the abdominal peritoneum and organs. From the cœliac ganglia, besides its proximal connections and its connections with the two other ganglia above described, are given off branches to all the organs supplied by the cœliac artery. Along its course are found the following plexuses: gastricus, hepaticus, and lienalis.

The mesenteric ganglion belongs to the mesenteric plexus. Branches from this plexus accompany the branches of the superior mesenteric artery, and have a similar distribution. Fine filaments also connect these ganglia with the plexus aorticus thoracalis and abdominalis.

(*c*) *The Splanchnic.* The most important connections of the cœliac ganglia with the central nervous system are through the splanchnic nerves. These are formed as follows:

Opposite the head of the thirteenth rib is a fusiform swelling of the gangliated cord which extends somewhat above and somewhat below the costovertebral junction. This swelling divides into two main branches: a large anterior branch (the splanchnicus major), and a finer branch (the splanchnicus minor). The right splanchnicus major courses caudad into the abdominal cavity, appearing under the ligamentum arcuatum externum of the diaphragm. Here it lies behind the liver. It then turns inward behind the kidney, the adrenal, and the vena cava. Behind the adrenal it communicates with the suprarenal plexus, in some instances continuing by its main branches directly upward into the root of the mesentery to join the right cœliac ganglion; in other instances joining more directly with the superior mesenteric ganglion. Sometimes the connections of the splanchnic with both ganglia are equally pronounced. The left splanchnic appears in the abdominal cavity similarly, but is covered by the stomach. It curves medial, parallel with and slightly above the first lumbar vein to the upper pole of the adrenal. Here fibers are given off to the adrenal plexus, its main trunk continuing upward into the left cœliac ganglion.

The Lumbar Sympathetic Ganglia. The cœliac and mesenteric ganglia communicate with the lumbar portion of the gangliated cord through the adrenal plexus. The latter may be connected with from two to four lumbar ganglia. The filaments from the lumbar ganglia to the adrenal plexus have been termed splanchnicus minor by a number of authors (Noellner, Ellenberger u. Baum); but in the description here given the term "splanchnicus minor" is limited to the smaller of the two terminal branches of the gangliated cord. From a number of dissections we have constructed a diagram (Fig. 1) to show the connections above described.

The Central Nervous System. Through the splanchnic nerves and the lumbar ganglia these plexuses receive fibers from the spinal cord by way of the rami communicantes. These contain both white and gray fibers. The former are derived from the central nervous system, the latter from the sympathetic. Each communicating

ramus connects a ganglion of the gangliated cord with the ventral branch (ramus ventralis) of the corresponding spinal nerve. Inasmuch as our knowledge of the centers of the splanchnic nerve, and of the rest of the gangliated cord, is not beyond dispute, absolutely definite central stations for the large abdominal sympathetic ganglia cannot be given. The more important opinions are the following:

Biedl, who investigated the location of the splanchnic center, found that it was not limited to a sharply defined area, but that cells were distributed from the sixth cervical down to the fifth thoracic segments. The ganglion-cells constituting the center are situated in the gray matter of the cord. The origin of the other fibers found in the sympathetic lies, according to one view, in the anterior and posterior horns; according to another view, also in the cells of Clarke's column.

Classification of Fibers of the Sympathetic. The fibers of the sympathetic are divided by Kölliker into non-medullated and medullated. The non-medullated arise from the sympathetic ganglia themselves, and are wholly motor. They supply all the smooth-muscle fibers. The medullated fibers arise in part from the anterior and in part from the posterior horns. Those from the anterior horns either go directly to their destination (the vasodilators and viscero-inhibitors) or terminate in ganglia. In this case their path is indirect, the impulses being taken up by non-medullated fibers of the ganglia (vasoconstrictors and visceroconstrictors).

Finally, there are the medullated fibers from the posterior horns. These are sensory, and go directly to their destination. According to this view, there are no sensory fibers which belong, strictly speaking, to the sympathetic system; the only sensory fibers in it are cerebrospinal. Running in the sympathetic system are found two sets of fibers which really belong to the cerebrospinal system: (*a*) The sensory fibers from the posterior horns; (*b*) The vasodilators and viscero-inhibitors from the anterior horns.

Other investigators, however, take the view that sensory (afferent) fibers also originate from the plexuses. Onuf and Collins have traced these to cells in Clarke's columns and in the intermediate zone.

2. *The Cerebrospinal Nerves*

Of these, there are the five or six lower thoracic nerves supplying the diaphragm (Ellenberger u. Baum) and the branches from the intercostal, lumbar, and sacral nerves which supply the parietal peritoneum. According to Lennander, it is these which carry pain impulses from the parietal peritoneum.

The Vagus. Through its dorsal and ventral branches it forms, after its passage through the œsophageal opening in the diaphragm, the anterior and posterior gastric plexuses. The former distributes fibers along the lesser curvature and ventral surface of the stomach and liver. The latter supplies the dorsal surface of the stomach and communicates with the cœliac plexus.

The Phrenic. This nerve, though usually considered purely motor, is thought by some authors to contain sane sensory fibers, which are distributed in the abdomen to a small portion of parietal peritoneum and to the diaphragm. Von Ziemssen, from the study of a clinical case in which he was able to carry out electrical stimulation of the exposed phrenic on the left side, considers the phrenic nerve purely motor. He found that neither pain nor sensation of any sort was felt upon irritation of the phrenic nerve, even when strong currents were employed, and that therefore Lusehka was wrong in maintaining that the phrenic sends sensory fibers to the pericardium, pleura, and peritoneum.

THE TECHNIQUE OF THE EXPERIMENTS

Before operation the animals were observed; the character of their stool and their weight noted. In order to insure an empty condition of the intestines, and particularly of the stomach, which, if distended, gives rise to considerable difficulty during the operation, food and drink were withheld for at least twelve hours before the experiment. Usually this sufficed; but in a number of the dogs so treated the stomach was found sufficiently filled to cause some annoyance during the operation. The abdomen was completely shaved, from 6 cm. above the xiphoid cartilage down to the pubis, several hours previously. No morphine was given; ether, in the smallest possible amounts, being regularly used for anæsthesia. The usual preparation of the abdomen (thorough scrubbing, ether, bichloride) was carefully carried out in each instance.

Operation. A longitudinal incision 7–9 cm. in length was made about 1 cm. to the left of the median line, over the rectus, its upper extremity reaching to within a short distance of the border of the ribs, where it was continued horizontally inward to the median line. The rectus was then incised down to the peritoneum and cut across at its upper end. A large split compress was put over the field of operation, the peritoneum incised, and its cut edges carefully adapted to the compress by means of Mikulicz clamps, thus completely excluding the skin from the operative field. The next step was the exposure of the left cœliac ganglion. This was accomplished by packing off the small intestines, pancreas, and mesentery to the right, stomach and spleen upward into the left hypochondrium, where they were held by hot compresses and large, broad retractors. At the bottom of the field thus exposed, the left kidney and adrenal came to view. These were dislocated downward by the fingers of the operator's left hand; a maneuver which, although not always necessary, is essential when the ganglion is situated high. The retroperitoneal tissues and peritoneum are pulled downward by it, bringing into view the left cœliac ganglion, slightly above and medial to the left adrenal, shimmering through the tense peritoneum. A tiny incision was made to the left of the ganglion, the rent enlarged by blunt dissection, and the ganglion lifted up by means of a small hook. In this way the nerve connections, to be found, in the main, at the upper and lower poles, were brought into relief; they were denuded of their peritoneal covering and cut across with long curved scissors. This was accomplished, as a rule, with very slight hemorrhage, and the application of artery-clamps was

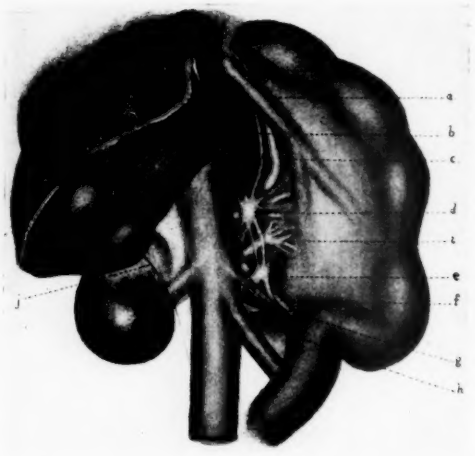

Fig. 2. Topography of right cœliac and superior mesenteric ganglia in the dog. The duodenum is drawn over to the left. Somewhat diagrammatic, to make the ganglia more prominent. Liver displaced slightly upward.

a. Portal vein.
b. Ligamentum hepatogastro-duodenale.
c. Hepatic artery.
d. Right cœliac ganglion.
e. Superior mesenteric ganglion.
j. Mesenteric artery.
g. Upper pole of left adrenal.
h. Upper pole of left kidney.
i. Distended chyle-ducts.
j. Ligamentum hepato-renale.

not required. It was only in those instances in which the lower pole of the ganglion was connected with the superior mesenteric ganglion by a short, broad strand or almost fused with it that these structures had to be liberated from the upper pole of the adrenal. There one encounters small vessels which may have to be tied. When the superior mesenteric ganglion lies in such close proximity, a large part of it may be easily pulled into view during the dissection, giving the impression that there are two ganglia on the left side.

For the exposure of the ganglia on the right side, after removal of the retractors and compresses the greater part of the abdominal contents must be dislocated to the left. Working now from the right side of the animal, the operator packed off the small intestines — some downward, but mostly to the left — where they were temporarily held in the usual way. The liver and stomach above and the kidney at the bottom of the wound are the structures which now present. By means of two fingers of the operator's left hand, the right lobe of the liver was lifted from the anterior surface of the kidney, and the fine, membranous, non-vascular ligamentum hepatorenale put on the stretch and severed. This step, if regularly carried out, prevents the disagreeable tears in the liver which usually result from attempts to dislocate it from the operative field. A single deep retractor and packing served to hold the liver and stomach in place. The temporary retractor holding the intestines was now removed, and the operator was able by manipulations with his right hand to put on the stretch the right layer of the mesentery of the duodenum in such a way as to bring into view the free fold of peritoneum carrying the hepatic artery. This was made and held tense by packing and retractors. Coursing down through the middle of the operative field the vena cava could now be seen, just medial to which the right cœliac and superior mesenteric ganglia must be sought for. The former was first removed by a technique similar to that already described for the left side. By keeping close to the ganglion itself, the hepatic artery was easily avoided. The superior mesenteric ganglion may be more difficult to find; but it can usually be detected if the fold of mesentery containing the artery be used as a guide and the peritoneum put on the stretch. It was never found necessary to ligate a vessel in the removal of these two ganglia.

The peritoneum was closed with a running silk suture, the omentum having been placed so as to lie directly under the wound. The rectus and skin were sutured in two layers by interrupted silk. Sterile gauze and adhesive plaster dressing. The complete time of operation varied from thirty minutes in the small to one hour and fifteen minutes in the larger dogs.

The incision on the left side was chosen, both because if affords easier access to the ganglia and because that triangular flap of fatty tissue which is suspended from the peritoneum of the epigastrium and lies to the right of the median line is thus wholly avoided. The small cross-incision facilitates the exposure considerably. It was found wise not to attack all the ganglia from the same side. Such a procedure would necessitate going through the thickness of the mesentery, and would result in the injury of a large number of blood and chyle vessels. In proceeding from the left side to the right, one has to throw over to the left all those structures which lie in the mesentery interposed between the ganglia of the two sides; namely, the pancreas in the mesoduodenum, and the mesentery of the small intestine and duodenum.

Complications. Firstly, we may mention the danger of establishing a pneumothorax if the abdominal incision be carried too high up, or if the cross-incision be made at too high a level. Just in the median line in the epigastrium the diaphragm may descend rather low, and in two cases (out of all our laparotomies) the pleural cavity was opened. This was combated by immediate clamping and suture. Secondly, the wounding of the large chyle-vessels in the root of the mesentery, particularly when they are distended. It is important, in the removal of the left cœliac ganglion, to work from its lateral aspect in the dissection, since the chyle-ducts lie median and caudad to it; in the removal of the other ganglia it is at times impossible to avoid wounding some of these channels. Lastly, some authors have regarded the opening of the pleural cavity during the extirpation of the left cœliac ganglion as a very likely complication. This accident did not occur in our experiments.

THE RESULTS OF THE EXPERIMENTS

We have found that *the cœliac and mesenteric ganglia are not essential to life in dogs, and their removal is neither attended by evidences of shock nor followed by constant physiological disturbances. The disturbances that were observed were too variable to warrant any general conclusions as to the physiology of the ganglia.*

Nine dogs were operated upon. In seven, the superior mesenteric and the two cœliac ganglia were completely removed; in two, only the superior mesenteric and the left cœliac. All nine dogs survived the operation. Some of these were able to run around immediately after the operation (e. g., Dogs 8, 9), and all of them were active and in good condition within three hours after leaving the table. In no case was there any collapse. On the day following operation, the dogs ate well, and in most cases they were lively and ran about quite normally. Save for the evident loss of weight which immediately follows the operation in most instances, the dogs appeared perfectly normal on the second day.

Duration of Life. Two dogs died of acute intussusception after four and six days, respectively. Three others died spontaneously after 3, 6½, and 10 weeks. One dog was still living 13 weeks after operation; and the three others were killed after 10, 10½, and 11½ weeks, respectively. Four dogs, therefore, were living and in good condition from 2½ to 3 months after operation.

If we turn our attention to the seven animals which did not succumb to a complicating intussusception, we find nothing of note, save for the occurrence of diarrhœa and change in body weight. The temperature during the first week was in all cases practically normal; the urine, examined only in two cases during the first week, was negative for albumin, sugar, and acetone.

Diarrhœa followed shortly after the operation in four instances. In three of these, there was a large fluid movement on the second day, which, however, was the only evidence of intestinal disturbance, the movements after that being either formed or semisolid. In one animal, diarrhœa was the rule, commencing on the fourth day, persisting till the ninth day, and reappearing at the end of the second week. Even in this animal the movements were formed after the third week. In the other three dogs, fluid stools were but occasionally observed throughout the course.

Loss of weight was observed in all of the animals. A number of them, however, either wholly or nearly regained their original weight after a number of weeks, and appeared perfectly normal. Two of these then began to decline rapidly and to emaciate 4½ and 7½ weeks after the operation.

Having noted the general features common to the above series, let us analyze the course taken by those animals which died after 3, 6½, and 10 weeks, and inquire into the cause of death. One of these (Dog 8), a very small, young animal of 2.50 kilos, was in excellent condition for almost two weeks after total extirpation, and had practically lost no weight. In the third week, he began to decline rapidly, lost weight, and died at the end of three weeks. The autopsy revealed no lesions in the organs, except a very marked catarrhal enteritis. In view of the good condition subsequent to operation, the sudden change in the third week, the slight resisting powers of the animal, and the pathological findings, we may assume that in all probability death was due to a complicating inflammatory condition of the intestines. A second (Dog 5) remained also in good condition until the fifth week, having up to that time lost but 0.4 kilo. He then began to decline and lose weight rapidly, and died after six weeks. The autopsy showed no lesions referable to the extirpation of the ganglia. Striking, only, was the presence of a large amount of serous fluid in the peritoneal cavity and marked scarring in the mesentery at the site of operation. Inasmuch as the chyle-ducts were not injured at the operation and the fluid was not chylous, it is highly improbable that obliteration of the duct had taken place. The third (Dog 7), a strong, healthy animal, showed no abnormality for six weeks; then rapid loss of weight, emaciation, and death after the tenth week. At the autopsy the only marked lesions were several ulcers in the duodenum, such as we have seen in many dogs in which an extirpation had not been done. The exact cause of death could not be determined.

Intussusception was a fatal complication in

two animals of this series. In the first (Dog 1), death occurred after six days, the intussusception being confined wholly to the small gut. The intussusceptum was partly gangrenous. In the second (Dog 9), a small animal, which was in unusually good condition for forty-eight hours after operation, and which died after four days, a similar condition was found. Here the ileum was invaginated into the colon, having descended to within a short distance of the anus. The intussusceptum was gangrenous throughout, and, in the absence of any other pathological changes, was regarded as the cause of death.

The autopsies performed upon all the animals, except one, showed no traces of the ganglia which had been attacked at the operation. In the animals which were killed, 10 to 11½ weeks after operation, the pathological changes were very meager. The wounds were completely healed, and there was usually very slight adhesion of the omentum to the wound. The peritoneum showed no changes, except slight adhesions of the right layer of mesoduodenum to the kidney and of the gastrosplenic omentum to the site of operation on the left side. At the site of the removed ganglia there was dense scar tissue, into which the main fibers of the splanchnics could be traced. In Dog 6, which died in emaciation, the organs showed the evidences of malnutrition. In two animals there were a few ulcers in the small intestines; in the third, none. Ulcers in the stomach were never observed. The other organs were negative.

General Considerations and Summary. It becomes evident from our study that the physiological disturbances and pathological changes which follow extirpation of the main abdominal sympathetic ganglia in dogs are surprisingly slight. The organs were not studied histologically for finer changes, inasmuch as Strehl, in an extensive study on cats, had found practically none. Although we have positive evidence that the life of animals can continue without these ganglia, we are unable to attribute the minor changes observed to the absence of the ganglia. Diarrhœa, when it occurred, was temporary. Inasmuch as it was found absent in large series of dogs, in which the intestines were similarly manipulated, but in which the ganglia were not extirpated, we may conclude that removal of the ganglia is usually followed by temporary disturbance in the motility of the intestinal tract. As for the loss of weight, it was also noticed in dogs laparotomized for other purposes, and may have been purely an accident of confinement. That permanent impairment of the intestinal function is caused by the extirpation of the ganglia, cannot be inferred from the *data* at our disposal. The post-operative glycosuria and acetonuria reported by some observers has been found absent by so many, that we deemed it inadvisable to study this point. Ulcers in the intestinal tract, as we have indicated before, occur so frequently in the dog that they must be regarded rather as accidental complications.

What relation, if any, has the occurrence of intussusception to the extirpation of the ganglia? Its appearance in two cases out of nine operated on is indeed suggestive. It occurred, however, in two other dogs whose ganglia were not extirpated.

The first was a dog in which an attempt was made to expose the ganglia on the right side for the purpose of stimulation. Owing to the length of the thoracic cavity, the high situation of the ganglia, and the distention of the intestines, a successful exposure was impossible. After thirty-five minutes the abdomen was closed and the dog kept under observation. The animal succumbed after 3½ days to acute intussusception. (See Protocol Dog 10.) It is evident, therefore, that intussusception can follow simple laparotomy. The second animal belongs to our third series, and will be later discussed. Suffice it to say here that this dog remained healthy for eight days after laparotomy and the induction of an experimental peritonitis; and died on the twelfth day, with a very extensive intussusception. (See Protocol Dog 34.)

PROTOCOLS

In the following protocols are given the more important *data* of this series of experiments. For the sake of clearness the experiments are considered in three groups: 1. Partial Extirpations of the Ganglia; 2. Complete Extirpations of the Ganglia; 3. Extirpations Followed by Acute Intestinal Intussusception.

Partial Extirpations

Dog 2. *Extirpation of the Left Cœliac and Superior Mesenteric Ganglia.* Male, fox-terrior; 8,300 k. January 9, 1906. Usual operation; duration 1 hour; 1 hour after operation, in good condition; no shock. Diarrhœal

movement second day; later, formed stools. Killed at the end of 10 weeks; condition good.

Dog 3. *Extirpation of the Left Cœliac and Superior Mesenteric Ganglia.* Male, fox-terrier; 7,750 k. January 12, 1906. Usual operation; duration 65 minutes; no shock. No diarrhœa during first week; slight diarrhœa second week. Killed at the end of 10½ weeks; slightly emaciated; 6,200 k.

Complete Extirpations

Dog 4. Mongrel, female; 6,250 k. January 16, 1906. Usual operation; duration 65 minutes; no shock. Fourth, eighth, and ninth days, diarrhœa; later, variable; after 13 weeks, apparently normal. Dog escaped from confinement.

Dog 5. Male, mongrel; 7,750 k. January 19, 1906. Usual operation; duration 70 minutes; no shock. Diarrhœa on first, sixth, and tenth days; again during the third week; in good condition until fifth week; then rapid emaciation. Death after 6½ weeks. *Autopsy:* Moderate ascites; adhesions at the site of ganglia, no trace of which could be found.

Dog 6. Female, basset; 7,300 k. January 23, 1906. Usual operation; duration 75 minutes; no shock. Fourth and sixth days, diarrhœal movements; also at the end of second week; later, formed stools. Killed after 11½ weeks.

Dog 7. Male, mongrel; 8,500 k. January 26, 1906. Usual operation; duration 75 minutes; no shock. Second day, diarrhœal movement; good condition until seventh week; then declines rapidly, and dies after 10 weeks.

Dog 8. Female, Skye terrier; 2,500 k. February 6, 1906. Usual operation; duration 35 minutes; no shock. Second day, diarrhœa; good condition during first week; after the twelfth day, rapid decline. Death at the end of the third week. *Autopsy:* Catarrhal enteritis.

Complete Extirpations Followed by Intussusception

Dog 1. Male, hound; 7,500 k. December 20, 1905. Usual operation; duration 75 minutes; no shock. Third, fifth, and sixth days, diarrhœa; in good condition until fourth day; rapid decline. Death after 6 days. *Autopsy:* In the upper part of small intestine about 15 cm. of the gut invaginated; beginning gangrene of the intussusceptum.

Dog 9. Male, Skye terrier; 1,900 k. February 8, 1906. Usual operation; duration 30 minutes; no shock. Good condition during first 2 days. Death 4 days after operation. *Autopsy:* Ileocolic intussusception; intussusceptum gangrenous throughout three fourths of its extent.

We wish to include here the protocol of an experiment which bears directly on the conclusions which one would be apt to draw from the occurrence of acute intestinal invagination in the two above extirpation experiments. (Dogs 1, 9.) That invagination may follow simple laparotomy with manipulation of the intestines may be seen from the following:

Intussusception Following Simple Laparotomy

Dog 10. Female, fox-terrier; 7,530 k. March 6, 1906. Usual incision and attempt to expose ganglia on the right side; unsuccessful; duration 45 minutes; in good condition after operation. No movement after second day. Death 3½ days after operation. *Autopsy:* 55 cm. above the ileocœcal junction is the distal end of an intussusception 23 cm. in length.

II. The Electric Stimulation of the Cœliac and Mesenteric Ganglia

Our study has led us to the following conclusion: *That galvanic and faradic stimulation of the cœliac and superior mesenteric ganglia does not alone suffice to produce a condition of abdominal shock.*

The condition usually known as abdominal shock is held by many to be reflex, and due to impulses which, arising from the peritoneum, pass through the sympathetic nervous system. If we could experimentally produce such a condition by way of the sympathetic, this theory would have direct substantiation. Thus far, positive evidence has been obtained only in the case of the frog, and the results of the work on warm-blooded animals has been negative. In attacking the problem, the repeated electrical stimulation of the large central stations of the abdominal sympathetic seemed to us worthy of a trial.

Most of the *data* in the literature bear upon the question as to whether the irritation of the ganglia will elicit pain. Thus Longet noticed manifestations of pain upon galvanization of the semilunar ganglia in dogs. Wutzer (cited by Buch) observed no signs of pain upon mechanical irritation of the abdominal sympathetic ganglia in dogs, but noticed marked pain with electrical stimulation. Other older authors (Mayer and Müller), as well as more recent investigators (Lewin, Boer, and Peiper), also noticed evidences of pain when the ganglia were manipulated or insulted. On the other hand, Magendie regarded the ganglia as devoid of sensation, and Bichat could produce no pain by the application to them of acids.

In an experimental study of the conditions which predispose to the production of abdominal shock, Guinard and Tixier investigated the centripetal effect of irritation of the viscera. They noticed a fall in blood-pressure and a

slowing of the pulse after various sorts of manipulation of the eviscerated intestines. Their work seems to speak for the existence of some relation between the sympathetic and abdominal shock.

THE TECHNIQUE OF THE EXPERIMENTS

The general preparation of the animals was the same as that described for the extirpation experiments. Here, also, ether was used, but only sufficient for the exposure of the ganglia. The animal was then allowed to come out of narcosis, in order that the immediate effects of the stimulations might be observed. A few whiffs of ether were given whenever the dog became unmanageable. The ganglia on the side to be stimulated were exposed as if for extirpation. A slight rent was made in the peritoneum, and the ganglia were lifted up from the retroperitoneal tissues by careful blunt dissection. They were held in place by guy-ligatures fastened to the abdominal wall. The greatest care was taken throughout to avoid injury to them; they were never grasped with forceps, but were manipulated only with blunt hooks. Throughout the experiment an assistant kept the operative field clear by broad retractors, leaving one of us free to apply the electrodes, and the other free to manipulate the electrical apparatus and to make the observations. Two sterilized copper electrodes, insulated except at their hooked extremities, were employed; one when the stimulation was limited to the left cœliac ganglion, and both when the right cœliac and superior mesenteric ganglia were attacked. Passed under the ganglia, they served to hold them up away from the other tissues, so that the electric current was applied to them alone. The period of stimulation over, the abdomen was closed as already described.

The faradic current employed was somewhat stronger than could be comfortably borne when applied to the human ulnar nerve, and was obtained from a street current of 110 volts, regulated by an electric table. One large, flat electrode was applied to the sternum, and the other to the ganglion.

When the galvanic current was used, the negative electrode was applied to the ganglion, except in one instance, in which both electrodes were applied to the left cœliac ganglion. A rather strong current of about twenty milliamperes, through the same electric table was employed.[1] When the circuit was kept closed for some time, there was considerable electrolysis and burning of the exterior of the ganglion. When interrupted shocks were given, this could be partly avoided by frequently changing the position of the electrode.

THE RESULTS OF THE EXPERIMENTS

There were ten dogs in this series; five were stimulated by the faradic and five by the galvanic current. The stimulations were continued over periods of time varying from ten to forty-eight minutes, the number of stimuli and duration of contact varying in each experiment, as detailed in the protocols.

Upon application of either kind of current there were marked evidences of pain. These varied considerably in the different dogs, but were present in all but one. It was impossible to make exact observations on the sensibility of the ganglia, because the degree of anæsthesia was inconstant. The simple manipulation of the ganglia, as seen also in the extirpation experiments, was sometimes accompanied by evidences of pain; but this was absent in certain of the dogs, even though they were out of anæsthesia. It was particularly on closure of the current that pain was manifested, and was more marked in the galvanic than in the faradic stimulations. With each cathodal closure the animal yelled, but became quiet immediately upon breaking the current.

In eight of the animals there were no evidences of collapse, either during or after the operation. Certain changes in the pulse-rate were noted in four of the five faradized animals. In two instances a drop of about sixty to eighty beats occurred during the first five minutes of stimulation. (Protocols Dogs 16, 17.) This fall was only temporary, the pulse gradually rising again to reach the previous level twelve to twenty minutes after the last stimulation. In three others the drop was slight and temporary. This fall in pulse-rate was not noticed when the galvanic current was used.

Eight animals survived, and were killed from four to nine weeks after the operation. In most of them there was some loss of weight. The stools remained normal, the condition good, and the animals could be considered as wholly unaffected by the experiment. At the autopsies of these animals, no lesions were found, except for some cicatricial change and adhesions at the site of the stimulated ganglia.

Two dogs died, one on the table and the other during the night. The former (see Protocol Dog 18) died suddenly after having received fifty-eight galvanic stimulations. In this animal the pleural cavity was accidentally opened, and the hole immediately closed by suture. A certain amount of air had, however, entered, and we are unable, therefore, to exclude pneumothorax as a cause of death.

[1] Except in experiment with weak current.

The second animal was in very poor condition at the beginning of the experiment, was apathetic throughout, and gave little or no evidence of pain. We were not able to conclude that its death was brought about by the irritation of the ganglia.

PROTOCOLS

The protocols of this series of experiments will be given in two sets: (*a*) The Faradic Stimulations, and (*b*) The Galvanic Stimulations.

(*a*) *Faradic Stimulations*

Dog 11. *Faradization of the Left Cœliac Ganglion.* Female, fox-terrier; 5 k. February 14, 1906. Seven short and two long (6 minutes each) applications of the current; duration of stimulation, 48 minutes;[1] duration of experiment,[2] 1 hour and 44 minutes; marked evidence of pain throughout experiment; no shock; after operation, condition good. After third week, rapid loss of weight. Killed after 4 weeks. *Autopsy:* Organs negative, except for small intestines, which showed small hemorrhages in the mucosa.

Dog 14. *Faradization of the Left Cœliac Ganglion.* Male, basset; 780 k. February 23, 1906. One short and four long (5 minutes each) applications; duration of stimulation, 36 minutes; duration of experiment, ½ hour; marked evidence of pain; some slowing of the pulse after 15 minutes of stimulation; no shock; condition good after operation. In good condition 8 weeks after operation. Then killed.

Dog 15. *Faradization of the Right Cœliac and Superior Mesenteric Ganglia.* Female, fox-terrier; 5,000 k.; in very poor condition. March 2, 1906. Three long applications (5 minutes each); duration of stimulation, 30 minutes; of experiment, 1 hour and 20 minutes; marked apathy throughout experiment; only slight evidence of pain; apathetic condition persists after operation. Death during night. *Autopsy:* Cause of death not determinable.

Dog 16. *Faradization of the Right Cœliac and Mesenteric Ganglia.* Female, fox-terrier; 8,250 k. March 13, 1906. Two long applications, 6 and 5 minutes, respectively; duration of stimulation, 30 minutes; of operation, 1 hour and 15 minutes; marked evidences of pain; no shock; pulse, marked drop in rate after first stimulation; complete return not until after cessation of stimulation. Killed 7 weeks after operation; in good condition.

Dog 17. *Faradization of the Right Cœliac and Mesenteric Ganglia.* Female, mongrel; 10,250 k. March 14, 1906. Two long applications, 10 minutes each; duration of stimulation, 30 minutes; of operation, 1 hour and 30 minutes; some evidence of pain; no shock; pulse drops after the first stimulation; then gradual rise. Killed 6½ weeks after operation; in good condition.

(*b*) *Galvanic Stimulations*

Dog 12. *Galvanization of the Left Cœliac Ganglion.* Male, hound; 8,850 k. February 16, 1906. Six short and two long (2 minutes each) applications; duration of stimulation, 13 minutes; duration of experiment, 1 hour and 20 minutes; marked pain; pulse, slight slowing after six short applications. Killed 9 weeks after operation; in good condition.

Dog 13. *Galvanization of the Left Cœliac Ganglion.* Male, fox-terrier; 6,700 k. February 17, 1906. Five short and four long (1 to 5 minutes) applications; duration of stimulation, 20 minutes; of experiment, 1 hour and 25 minutes; moderate pain. Killed 9 weeks after operation; in good condition.

Dog 18. *Galvanization of the Right Cœliac and Superior Mesenteric Ganglia.* Female, mongrel; 5,750 k. March 16, 1906. 58 k.c.;[3] duration of stimulation, 10 minutes; marked pain; dog dead 3 minutes after a series of 50 stimulations had been given. When opening the peritoneum, a slit was made in the pleura, and a small amount of air entered. The slit was, however, immediately clamped and sutured. *Autopsy:* Negative.

Dog 19. *Galvanization of the Right Cœliac and Mesenteric Ganglia.* Male, fox-terrier, 8,800 k. March 20, 1906. 500 k.c.; duration of stimulations, 25 minutes; duration of operation, 1 hour and 25 minutes; marked pain. Killed 5 weeks after operation; in good condition.

Dog 20. *Galvanization of the Right Cœliac and Superior Mesenteric Ganglia.* Male, Skye terrier; 5,000 k. March 21, 1906. 1,050 k.c.; duration of stimulation, 37 minutes; of experiment, 1 hour and 30 minutes; marked pain. Killed 5 weeks after experiment; in good condition.

STIMULATION OF THE SPLANCHNIC NERVES

In a short series of three animals, stimulation of the splanchnic nerves was undertaken. It was our object to see whether the direct irritation of the large nerve-trunks themselves would be more efficacious than similar stimulation of the ganglia. Considering the intimate anatomical association of these structures, and the fact that a strong current had been used for the irritation of the ganglia, it appeared likely that these experiments would also be negative in their results. We therefore observed only the immediate effects of the stimulation — pain, pulse-changes, and general condition — and killed the animals at the end of the experiment.

The reports in the literature seem to agree that the splanchnic nerves are capable of trans-

[1] By "duration of stimulation" is meant the time which elapsed between the beginning of the first and the end of the last application of the electrode.

[2] By "duration of experiment" is meant the time which elapsed between the beginning of anæsthesia and the suture of the skin.

[3] K.C. (= cathodal closing) means closure of the circuit, with the cathode on the ganglion.

mitting pain impulses. Friedländer stimulated the central end of the cut splanchnic in cats, and says: "Der Nerv ist überall schmerzempfindlich. Selbst wenn das Thier in Aethernarcose ist, wacht es bei electrischer Reizung des Splanchnicus auf, und wimmert vor Schmerz." From the negative nature of his experiments on the vagus, he reaches the following conclusion: "Indem der Vagus nicht im Geringsten schmerzempfindlich ist, so folgt daraus, dass der peritonitische Schmerz auf dem Wege des Splanchnicus dem Central organ mitgeteilt wird." From a review of the literature, Buch finds that the splanchnicus major is regularly sensitive to pain. Friedländer, Graham, Ludwig, and Cyon report a rise in blood-pressure following electric stimulation of the splanchnics. Most of the experimenters have limited their studies to observations on the motor functions of these nerves.

The Experiments

In two of the dogs employed, the ganglia had been extirpated some months previously. For the other experiment an unoperated dog[1] was used. In two the left splanchnic and in one the right were stimulated. The same incision, as already described, sufficed for the exposure of the left splanchnic nerve; but for the right side it was necessary to continue its upper end along the border of the ribs on the right side. The left splanchnic is easily found if its abdominal portion be sought at a point on the level of the oral pole of the adrenal, and somewhat lateral to that organ. After leaving the thoracic cavity, it curves toward the median line a short distance above that prominent vein which crosses the adrenal. A small rent was made in the peritoneum, and the nerve freed and held up with a blunt hook. By pulling on the nerve a part of its supradiaphragmatic portion was brought into view without entering the pleural cavity. The cathode was then applied. The exposure of the right splanchnic nerve is somewhat more difficult. The ligamentum hepatorenale was cut, the liver pulled aside, and the nerve sought just below its exit from under the diaphragm above the first lumbar vein. In two experiments the usual galvanic current (700 k.c.) was used, and in the third, the faradic current. Stimulation in these cases was kept up for thirty minutes.

[1] Dog 21 of our series.

There were absolutely no manifestations of collapse in any of the animals. Evidences of pain were very marked, especially with the galvanic current. The pulse-rate remained practically constant; the blood-pressure was not measured. The animals were in excellent condition at the end of the experiment.

III. Stimulation of the Ganglia in the Presence of Peritonitis

From a consideration of the above it becomes clear that we were unable to establish a condition of abdominal shock in the normal, healthy dog, either by the extirpation of the ganglia with the associated mechanical irritation, or by the direct application of the electric current to the sympathetic system itself. If reflex impulses play a part in the production of abdominal shock, failure to produce this condition experimentally could be theoretically due to the employment of stimuli which are different in character from the impulses traveling in the nervous system. We deemed it advisable, however, to continue the use of the electric current, because it can be controlled, as regards quality, quantity, and force, better than any other irritant known. The failure may also have been due to the absence of some predisposing factor, without which the nervous system remains resistant, and in the presence of which it is rendered more susceptible. Clinical evidence seems to suggest that a peritonitis may be such a predisposing factor. The association of collapse with peritonitis and with other acute abdominal affections (such as torsions of the omentum, twisted pedicles, etc.) is well known. The possibility that these conditions affect the nervous system in some way cannot be denied. Is there a greater liability to shock in the presence of peritonitis? and can we produce it experimentally in animals in which a peritonitis already exists?

Our task, therefore, was: *The electrical stimulation of the abdominal sympathetic ganglia in dogs in which a peritonitis had been experimentally produced.*

We have found that by the irritation of the ganglia it is possible to produce shock, and even

death, in animals in which an experimental peritonitis due to turpentine already exists.

The Production of the Peritonitis. Turpentine was employed throughout for this purpose. It was selected because it causes intense inflammatory reaction, and could be introduced without adding bacteria and their toxines. If we had had to deal with a pure bacterial peritonitis, the local action of the organisms, their invasion of the circulation, their toxines and endotoxines, would have had to be considered. The use of turpentine simplified the experimental conditions. We are well aware that the secondary invasion of the peritoneum by bacteria from the intestinal tract cannot be entirely excluded. We therefore decided to commence the stimulation of the ganglia a few hours after the injection of the turpentine. This interval sufficed for the production of an intense irritation of the whole peritoneum.

The Technique. The experiment included: 1. The injection of morphium and turpentine; 2. The interval for the development of the peritonitis; and 3. The stimulation of the ganglia. The morphium was given in the morning, usually after a short period of fasting. The doses used varied from 0.02 to 0.0375, and were proportional to the weight of the dogs. The morphium regularly caused a distinct drop in the pulse-rate, and sometimes vomiting if the dog's stomach was full. The intraperitoneal turpentine injection was given by means of a trocar about one hour later, and was followed in about forty-five seconds by evidences of marked pain and rigidity of the abdomen. This was, at times, intense enough to cause the animal to assume the position of prosthotonus. To allay the pain, it was in some cases necessary to give a few whiffs of ether. The persistence of evidences of pain varied considerably in different animals, some quieting down shortly after the injection, others groaning for hours. A number of the dogs showed marked weakness of the hind legs, and walked with an uncertain and ataxic gait; in others, these manifestations were absent.

The laparotomy was done 4½ to 5 hours after the turpentine injection; and this interval, as well as the duration of the operation, the intensity and number of stimulations, was practically the same in all the experiments. Upon opening the abdomen, peritonitis of various degrees was encountered; usually intense injection of the intestines and mesenteries and a large quantity of slightly bloody serous exudate were present. Sometimes a beginning fibrinous peritonitis was found. The exudate was completely removed by mopping with hot saline compresses, and the right cœliac and superior mesenteric ganglia were then exposed in the usual way for stimulation. Except in special instances, 1,550 closures of the galvanic current were used, and the stimulations were conducted through a period of about forty-five minutes.

In the course of our work it was found advantageous to conduct three separate series of experiments, using large, small, and moderate doses of turpentine. We will therefore group our report as follows:

A. Experiments with Large Doses (4 c.c. and over). Of the four dogs treated in this way, two died on the table and two shortly after the operation. Dog 22 succumbed after 550 stimuli had been given; Dog 24, after 1,550. Of the other two, Dog 25 died one hour and Dog 23 from four to six hours after the operation. Marked acute peritonitis with serosanguineous exudate was present in all four dogs. The pulse was uninfluenced in rate by the stimulations, but gradually became weak in each experiment; in Dogs 22 and 24, it was difficult to count after one hundred and three hundred stimulations had been given. The most marked feature was the failure of the dogs to respond in the normal way. Thus in contrast to the marked reaction in healthy dogs, these animals showed evidences of pain only in the beginning of the experiment. After one hundred shocks, they were either quite collapsed or gave only feeble responses at the beginning of each subsequent series.

We next investigated whether a similar effect would result in animals so injected, but in which simple laparotomy was done. The following experiment will illustrate: An animal, injected in the usual way, was operated on the exudate mopped out, the region of the ganglia exposed, and retractors and compresses allowed to remain in place. After the lapse of time usually occupied by the stimulations, the abdomen was closed. This animal (Dog 26) had the usual serosanguineous exudate, with a

beginning fibrinous peritonitis. It was apathetic during the operation and died during the night. How were we to explain the death of this animal? Here, acute collapse, such as was brought on by direct stimulation in the other animals, was absent; and it seemed most probable that too much turpentine had been used. We therefore proceeded to employ smaller doses.

B. Experiments with Small Doses (1–3 c.c., dilute). Two animals were injected with 1 c.c. of turpentine (diluted with water). One was stimulated in the usual way, and in the other, simple laparotomy with exposure of the ganglia was done. In neither animal was there any evidence of collapse after the experiment, and both were in good condition for more than three weeks after the operation, when they were killed.

In three other experiments, larger doses (2–3 c.c.) were employed. One of these dogs was already weakened by the turpentine injection. His condition became rapidly worse on stimulation, and toward the end of the operation he was almost moribund. He received less than the usual number of stimulations. His failure to respond and frequent regurgitation of black stomach contents made it seem likely that he would not survive. After the operation he recovered rapidly, and was in fair condition next day. Another (Dog 28) became moderately collapsed, and died the day after the experiment. A third dog, however, which received 3 c.c. of turpentine, remained in good condition on the table and survived. From this series we concluded that stimulation of the ganglia in dogs with a moderate peritonitis is variable in its effect. Evidences of collapse may be marked or wholly lacking.

C. Experiments with Moderate Doses (4 c.c., dilute). In this series, the peritonitis, although less intense than in the case of the large doses, was pronounced, either purely exudative or coupled with a beginning fibrinous process. The ganglia of two such animals were stimulated, with the following results: Both animals responded feebly or not at all during the latter part of the period of stimulation, and showed evidences of collapse at the end of the experiment. The effect was fully as marked with the weak as with the strong current. One (Dog 32) died during the night; the other (Dog 33), eight hours after the experiment.

A control animal (Dog 34) was injected with the same dose, the abdomen opened, and the site of the ganglia exposed for a time equal to the usual period of stimulation. In contrast to the above animals, this one showed no evidence of collapse, and was in good condition after the operation and on the following day. It remained apparently normal for ten days, finally dying of a complication (acute intussusception).

Experimental Peritonitis Followed by Stimulation of a Peripheral Nerve. The effect of the stimulation of a peripheral nerve in the presence of a peritonitis was studied in Dog 35. We deemed it important to perform a laparotomy and leave the abdomen exposed for the usual time; firstly, because the mopping out of the exudate is a factor to be considered, and secondly, because the mere performance of this operation may play a part in the production of collapse. The cathode was applied to the anterior crural nerve of the right side, and 1,550 stimulations were given. There were moderate evidences of pain throughout, but absolutely no signs of collapse, the animal being able to run about immediately after the operation. It remained in good condition, and was killed ten days later.

Experiments on Dogs with Complete Transverse Lesion of the Spinal Cord. The spinal cord was cut between the fifth and eighth dorsal segment, in order to eliminate the greater part of the sensory spinal nerves which supply the abdominal cavity. In such dogs, centripetal impulses from the abdominal cavity must pass, in the main, by way of the sympathetic. Of the other nerves of the abdominal cavity — the vagus and the phrenic — only the latter is supposed to contain a few sensory fibers.

TECHNIQUE OF THE EXPERIMENTS

This included: (*a*) The Section of the Spinal Cord; (*b*) An Interval for Recovery; and (*c*) The Experiment Proper.

(*a*) *The Section of the Spinal Cord.* This was carried out at slightly varying levels between the fifth and eighth dorsal segment, in order to throw out the lower six thoracic nerves which supply the diaphragm (8–13, inclusive). Incision slightly to the left of the spines of

dorsal vertebræ 6–9; skin drawn to the right, two dorsal spines ablated, and the arch of one vertebra opened with a chisel. The dura and spinal cord were now cut through transversely, usually at two levels, about 7 mm. apart. This was followed by a gush of cerebrospinal fluid and some bleeding, which was controlled by temporary tamponade. A few deep silk sutures for the muscles; careful approximation of skin edges. Duration of operation, 20–25 minutes. *Anæsthesia:* 0.035 morphium, one hour before operation; very little ether necessary.

(*b*) *The Interval.* Immediate palsy, with exaggerated knee-reflexes, followed the section. The operation was well borne, and of six animals thus treated, but one died from the immediate effects of the operation.[1] The most troublesome feature was retention of urine. Catheterization with bladder irrigation was regularly carried out.

(*c*) *The Experiment.* Except for one small animal (Dog 36), the dogs were in good condition before the experiment. The technique was the same as that already described for the stimulation experiments. In order to observe whether the section of the abdomen would cause pain, only a small dose of morphium was given, and ether was withheld. In none of the animals was there any evidence of pain when the parietes and peritoneum were cut; and in two only, a small amount of ether was necessary during the exposure of the ganglia.

THE RESULTS

The usual experimental peritonitis was induced in three animals, two of which were stimulated and one used as control (simple laparotomy). Here, again, the stimulation of the ganglia was followed by collapse. Dog 38 died twenty minutes after the beginning of stimulation; Dog 39, just as the stimulations were completed. Both of these reacted but feebly, and only during the first few shocks. Control dog (Dog 37), on the other hand, showed no evidence of collapse, was in good condition after the operation, and survived.

It was of interest to note whether the injection of turpentine in these animals would elicit pain. According to the views of some, the sympathetic system contains no sensory fibers of its own, those few sensory fibers which run in it being cerebrospinal. The latter are supposed by Kölliker to be sufficient to transmit the sensory impulses that originate from the abdominal viscera. In our animals, all sensory nerves were thrown out of function, except the phrenic and the sympathetic. Of the four animals, two gave absolutely no manifestations of pain after the injection of the turpentine; but in the other two, pain was not appreciably less than that evidenced by a number of animals with intact cords. We are unable to explain these contradictory manifestations, and our experiments are too few to warrant drawing any conclusion in regard to this particular point.

In order to determine whether the peritonitis was an indispensable factor in the production of the collapse observed, we next decided to investigate the effect of stimulation of the ganglia after injection of turpentine into the abdominal wall. For this purpose two experiments were done. In one dog a large amount of turpentine (8 c.c., pure) and in the other 2 c.c. (diluted with water) were injected into the right rectus muscle and under the skin of the abdomen. After the usual interval, laparotomy and stimulation of the ganglia were done. Both animals responded actively throughout, just as normal, healthy dogs, and both were in good condition at the end of the experiment. In spite of the extensive abscess formation, the first was in good condition five weeks after the operation; in this case the pus was evacuated by incision a few days after the experiment. The second remained well for a week, and died on the eleventh day. The abscess in this case had not been opened, and secondary infection may account for the death.

The following are the protocols of the experiments in this series:

PROTOCOLS

A. Experiments with Large Doses

Dog 22. *Experimental Peritonitis Followed by Stimulation of the Ganglia.*[2] Male, mongrel; 5,000 k. March 27, 1906.

A. M. Morphium; turpentine 4 c.c., intraperitoneal; marked pain; no collapse before operation.

P. M. *Operation:* Marked injection of peritoneum; considerable serosanguineous exudate; 550 stimulations; pulse weak and difficult to count after the first series of 100 shocks; also ceases to react with any evidences of pain; death on table, 37 minutes after incision, and after 550 shocks had been given.

Dog 23. *Experimental Peritonitis Followed by Stimulation of the Ganglia.* Male, shepherd dog; 7,200 k. March 28, 1906.

A. M. Morphium; turpentine 5 c.c., intraperitoneal; intense pain.

[1] This was a small dog, which died during the night.

[2] In this whole series, the right cœliac and superior mesenteric ganglia were simultaneously stimulated.

P. M. *Operation:* Intense injection of intestines; considerable serosanguineous exudate; 1,550 stimulations; pulse becomes weaker during this experiment; responds by evidences of pain only during the first series of 100 shocks; later, remains quiet throughout, apparently collapsed; after operation, marked collapse. Death between 4 and 6 hours after operation.

Dog 24. *Experimental Peritonitis Followed by Stimulation of the Ganglia.* Male, basset; 10,700 k. March 29, 1906.

A. M. Morphium; turpentine 6.5 c.c., intraperitoneal; intense pain.

P. M. *Operation:* Intense injection; considerable bloody serous exudate; 1,550 stimulations; pulse difficult to count after the first 300 stimulations; after the first 350, no more evidences of pain; marked collapse toward end of the experiment. Death immediately after the end of stimulations.

Dog 25. *Experimental Peritonitis Followed by Stimulation of the Ganglia.* Hound; 13 k. March 30, 1906.

A. M. Morphium; turpentine 8 c.c., intraperitoneal; marked pain.

P. M. *Operation:* Moderate amount of exudate; 1,300 stimulations; pulse becomes imperceptible at the end of experiment; reacts very little after first 100 stimulations; marked collapse after experiment. Death 1 hour after operation.

In the following experiment, the last of the series in which large doses of turpentine were used, injection of turpentine was followed by simple laparotomy, as already described.

Dog 26. *Experimental Peritonitis and Simple Laparotomy.* Male, terrier; 5,700 k. April 2, 1906.

A. M. Morphium; turpentine 4.1 c.c., intraperitoneal. Marked pain.

P. M. *Operation:* Marked peritonitis, beginning fibrin formation; no stimulations; pulse weak after closure of abdomen. Death during the night.

B. Experiments with Small Doses

Dog 27. *Experimental Peritonitis Followed by Simple Laparotomy.* Male, mongrel; 4,700 k. April 3, 1906.

A. M. Morphium; turpentine 1 c.c., water 4 c.c., intraperitoneal; moderate pain.

P. M. Operation, and exposure for the usual time, then suture; no shock. Killed 25 days after operation; in good condition.

Dog 28. *Experimental Peritonitis Followed by Stimulation of the Ganglia.* Male, mongrel; 5,500 k. April 6, 1906.

A. M. Morphium; turpentine 1 c.c., water 4 c.c., intraperitoneal; moderate pain.

P. M. *Operation:* Moderate peritonitis; 1,300 stimulations; pulse becomes weaker during experiment; no shock. Killed 24 days after operation.

Dog 29. *Experimental Peritonitis Followed by Stimulation of the Ganglia.* Male, terrier; 5,200 k. April 7, 1906.

A. M. Morphium; turpentine 2 c.c., water 3 c.c., intraperitoneal; marked pain.

P. M. *Operation:* Large amount of exudate; 1,550 stimulations; moderate collapse after the operation. Death next morning.

Dog 30. *Experimental Peritonitis Followed by Stimulation of the Ganglia.* Fox-terrier, male; 5,500 k. April 11, 1906.

A. M. Morphium; turpentine 2.5 c.c., water 4 c.c., intraperitoneal; marked pain.

P. M. *Operation:* Usual peritonitis; 1,050 stimulations; no response during the stimulations; marked collapse; improved in condition after experiment. Killed 17 days later.

Dog 31. *Experimental Peritonitis Followed by Stimulation of the Ganglia.* Male, fox-terrier; 6,300 k. April 18, 1906.

A. M. Morphium; turpentine 3 c.c., water 3 c.c., intraperitoneal; moderate pain.

P. M. *Operation:* Usual peritonitis; 1,550 stimulations; no collapse. Killed 10 days after operation.

C. Experiments with Moderate Doses

Dog 32. *Experimental Peritonitis Followed by Stimulation of the Ganglia.* Male, Irish setter; 8,700 k. April 12, 1906.

A. M. Morphium; turpentine and water, 4 c.c. each; marked pain.

P. M. *Operation:* Usual peritonitis; 1,050 stimulations; after the first 350, reacts little; later, not at all; moderate collapse. Death during the night.

Dog 33. *Experimental Peritonitis Followed by Stimulation of the Ganglia.* (Smaller current used; 5 milliamperes.) Hound; 8,200 k. April 20, 1906.

A. M. Morphium; turpentine and water, 4 c.c. each; moderate pain.

P. M. *Operation:* Usual peritonitis (some fibrin); 1,550 stimulations; pulse very weak towards end of experiment; collapse. Death next morning.

Dog 34. *Experimental Peritonitis Followed by Simple Laparotomy.* Hound; 6,800 k. April 14, 1906.

A. M. Morphium; turpentine and water, 4 c.c. each.

P. M. *Operation:* Usual peritonitis; exposure of the ganglia for the usual period; after operation, in good condition; following day in good condition. Death on the thirteenth day, from acute intussusception.

Dog 35. *Experimental Peritonitis Followed by Stimulation of the Right Anterior Crural Nerve.* Female, fox-terrier; 6,500 k. April 19, 1906.

A. M. Morphium; turpentine and water, 4 c.c. each.

P. M. *Operation:* Usual peritonitis; exposure of the anterior crural nerve and stimulation; from the evidence of pain on stimulation, no collapse. Killed ten days after experiment; in good condition.

Experiments with Moderate Doses. Section of the Cord

Dog 37. *Experimental Section of the Spinal Cord.* Peritonitis; control laparotomy. Male, collie; 14,800 k.

1. April 4, 1906. Section of cord, eighth dorsal segment.
2. April 11, 1906. Bladder drainage.
3. April 17, 1906. Intraperitoneal injection turpen-

tine and water, 4 c.c. each. Operation without stimulation of ganglia; no shock. Killed on sixteenth day.

Dog 38. *Experimental Section of the Spinal Cord.* Peritonitis; stimulation of the ganglia. Male, Irish setter; 13,500 k.

1. April 10, 1906. Section of cord, seventh dorsal segment.

2. April 18, 1906. Intraperitoneal injection turpentine and water, 4 c.c. each. Operation and stimulation, soon followed by collapse; death on table 20 minutes after beginning of experiment, and after 700 shocks had been given.

Dog 39. *Experimental Section of the Spinal Cord.* Peritonitis; stimulation of the ganglia. Male, collie; 13,500 k. April 19, 1906. Section of spinal cord, fifth dorsal segment. Operation a few days later, 1,550 shocks; death on table just as last stimulation had been given.

Control Experiments with Turpentine in the Abdominal Wall

Dog 40. *Intramural Injection of Turpentine Followed by Stimulation of the Ganglia.* Male, shepherd; 8,000 k. March 24, 1906.

A. M. Morphium; injection turpentine 8 c.c. into the right rectus and under the skin of the abdomen.

P. M. *Operation*: Peritoneum negative; 1,050 stimulations; active response throughout; in good condition at the end of operation. Killed after 5 weeks.

Dog 41. *Intramural Injection of Turpentine Followed by Stimulation of the Ganglia.* Male, fox-terrier; 6,500 k. April 9, 1906.

A. M. Morphium; injection turpentine 2 c.c., water 4 c.c., in right rectus and under the skin of the abdomen.

P. M. *Operation:* Peritoneum negative; 1,550 stimulations. Dog reacts throughout like normal animal; in good condition at the end of experiment.

SUMMARY

The results of our study of the stimulation of the ganglia in the presence of peritonitis are, then, briefly:

1. In the presence of a peritonitis due to small doses of turpentine, the reaction to electric stimulation of the ganglia may be the same as that of healthy normal animals. As the doses increased, however, evidences of collapse made their appearance.

2. When moderate doses were used, stimulation of the ganglia was regularly followed by marked evidences of collapse, and death either during the experiment or shortly afterward. Control animals in which simple laparotomy with exposure of the ganglia, or simple laparotomy with stimulation of a peripheral nerve, was done showed no signs of collapse, and remained in good condition after the experiment. The results of this series may be thus tabulated:

Experimental Peritonitis (4 c.c.)

With Stimulation of the Ganglia				*With Control Laparotomy*			
Dog 22		Collapse.	Died on table.	Dog 34		Good condition.	Lived.
Dog 32		Collapse.	Died during night.	Dog 35	Stimulation anterior crural nerve.	Good condition.	Lived.
Dog 38	Section spinal cord.	Collapse.	Died on table.	Dog 37	Section spinal cord.	Good condition.	Lived.
Dog 39	Section spinal cord.	Collapse.	Died on table.				

3. When small or even large doses of turpentine were injected subcutaneously and in the abdominal wall, the animals reacted in the normal way to stimulation of the ganglia, and survived the experiment.

GENERAL CONSIDERATIONS

How are we to account for the death of the stimulated animals and the appearance of the collapse? That death cannot be explained by toxic action of the turpentine alone is clear from the fact that the control animals survived. We must assume, therefore, that, inasmuch as normal animals are wholly unaffected by the stimulation, we have, *by the experimental production of peritonitis*, introduced a factor which has rendered the animals more susceptible to collapse. In our experiments the blood-pressure was not measured. The conception of the term "collapse" allows of such wide variations, and the diagnosis of the condition is so difficult in animals, that we had to limit the term to the most marked grades of this condition. Evidences of collapse were to be found in the following: 1. Fall in body temperature; 2. Condition of apathy; 3. Low blood-pressure, as evidenced by the pulse and slight bleeding; 4. Diminished pain sense; 5. Diminished or even total absence of reaction when stimuli were used which were sufficient to elicit constant and intense reflexes to normal animals; and finally, the inability to recover when subjected to repeated stimulation of their abdominal sympathetic ganglia.

In the observation of the animals, we at-

tempted to distinguish the untoward symptoms produced by the injection alone from those due to the stimulation. Thus in a number of animals, the pulse became weaker, the peritoneal contents somewhat cold, and the blood-pressure low before the stimulation was begun. Such a condition was not, in itself, significant of a fatal outcome, for it was not regularly present, and when present, was rapidly recovered from, in the animals which were not stimulated. The most striking effects were the production either of immediate death or a condition from which the animal could not recover.

It may be objected that the death in these animals was due to some of the following factors: Infection of the congested and inflamed peritoneum during the operation; secondary invasion of the peritoneum by organisms from the intestines; the combined effect of ether and turpentine upon the kidneys; and finally, such a weakening of the animals by the operation that they are unable to recover from their peritonitis. When we consider that some of the animals died on the table and that the control animals survived, these objections are seen to be groundless. We must therefore limit the factors which play a role in the production of death in these animals to the turpentine injection and the stimulation of the ganglia.

The introduction of the turpentine into the peritoneum seems to be an essential factor in the production of collapse and death, since the stimulation after intramural injections was wholly negative in its results. We may therefore assume that the presence of this irritant in the peritoneum has caused some alteration in the nervous system, and that this manifests itself in an abnormal susceptibility to reflex shock. This change could be theoretically explained as due either to the effect of a large quantity of absorbed turpentine or to nervous impulses from the peritoneal cavity. Although positive evidence could not be obtained in favor of either view, we nevertheless gained the impression from a series of experiments bearing upon this problem that absorption alone would not offer a satisfactory explanation.

We would therefore lean to the view that the stimulation of the ganglia was in some way or other responsible for the palsy of the higher centers in the medulla. The most likely explanation for the failure of these centers seems to us to be the following: *That the continued irritation of the nerve centers by way of the sympathetic gradually produced a condition of exhaustion incompatible with a continuation of their function.*

May not a similar mechanism account for some of the conditions of abdominal collapse clinically observed? In general, two theories have been advanced: the reflex and the toxic. In the experiments on the frog, the stoppage of the heart may be accounted for by reflex inhibition. In a recent publication, Heineke has ascribed collapse and death in peritonitis to the effect of the absorbed toxines on the centers in the medulla. Our own experiments, however, seem to furnish evidence that another modus, working neither by inhibition nor by intoxication alone, but by fatigue of the centers themselves, may sometimes obtain. Given a certain predisposition, continued nerve impulses sent from the irritated peritoneum by way of the sympathetic might also produce such a fatigue. A similar explanation might hold for the collapse attending long abdominal operations. On the other hand, the phenomena observed in our experiments seem to throw little light upon the method of production of the acute collapse, such as is occasioned by blows upon the abdomen and torsion of tumors and mesenteries.

Although we are well aware that our experimental work does not warrant drawing hard and fast conclusions, we feel that further experimentation along this line will lead to valuable information as to the role of nervous impulses in the production of abdominal shock.

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[1] We regret that at the time of writing this paper, the interesting work of Crile was not accessible.

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SURGERY OF TYPHOID PERFORATION [1]

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A BRIEF *résumé* of the history of this subject, with certain facts relating to typhoid fever, will set the matter before us in perhaps the clearest light possible.

FREQUENCY OF PERFORATION

Taylor, basing his estimate upon the census and Marine Hospital reports, says that five hundred thousand cases of typhoid fever occur annually in the United States. A safe estimate of the mortality of typhoid fever is ten per cent. Osler, who carefully studied the whole subject of typhoid fever at the Johns Hopkins Hospital, says that thirty per cent of the deaths occurring from typhoid in that hospital were due to perforation, and that even this percentage was greater when the mortality from general toxæmia was lowered. That thirty per cent of the mortality is due to perforation seems to be admitted by all investigators. Haggard, after reviewing many statistics, says that from two and a half to three per cent of the total cases of typhoid fever die from perforation.

Taking these figures as a basis, there are fifteen thousand or sixteen thousand people who die from typhoid perforation every year in this country. The personal question for each of us to ask is, "How many of them die in our community?"

SURGICAL HISTORY

Leyden, in 1884, was the first to suggest that typhoid perforation might be sutured. But Leyden's suggestion was heeded only by Mikulicz, who, the same year, did the first successful operation, which, however, was not published till James C. Wilson of Philadelphia, in 1886, wrote what Keen says is "the first published plea for operation in case of perfora-

[1] Read before the International Surgeons' Club, Rochester, Minnesota, September 21, 1906.

tion." Keen, in 1898, wrote an exhaustive brochure on the subject. Even to this time not very many papers have been written regarding the surgery of typhoid perforation. The most extensive bibliography I have been able to find is by Elsberg, in the *Annals of Surgery* for May, 1903, in which he cites fifty references from English and foreign literature, but many of these are simply reports of individual cases, so that perhaps not more than fifteen or twenty papers have ever been written upon this subject.

The number of operations for typhoid perforation is also surprisingly few. Keen, in 1898, was able to collect only 83 cases of operation for typhoid perforation, and Harte and Ashurst, in January, 1904, could find only 362 reported cases.

MORTALITY OF PERFORATION

The mortality of typhoid perforation is pretty nearly one hundred per cent. It is quite possible for an occasional case to get well without surgical interference, by reason of the perforation occurring slowly and circumscribing adhesions taking place, reducing the lesion to a local abscess. Osler says that he never saw a case recover without operation.

True, the mortality from operation is distressingly high, but it is decreasing all the time. Harte says the mortality of the first ten operations was 90 per cent, while the mortality in the last batch of 166 cases occurring just prior to January, 1904, was only 69.2 per cent.

Elsberg, in an able article in the *Annals of Surgery*, May, 1903, discusses perforation of the intestines in typhoid occurring in children, and cites 25 instances in which operation was done, with a mortality of only 36 per cent. It seems clear, therefore, from Elsberg's statistics that the mortality in children is less than half of what it is in adults. But even in adults I think it not too sanguine to expect that, with increased knowledge of the symptoms and importance of early operation, the mortality may, within a decade or two, be reduced to sixty or even fifty per cent.

TIME OF OPERATION

The prognosis is influenced more by the interval between the time of the perforation and the operation than by any other single factor. While the statistics furnished by Keen seem to indicate that better results were obtained from operation during the second twelve hours after perforation than the first twelve hours, yet this is easily explained by the fact that these cases were probably those who were in better general condition and in whom perforation probably occurred slowly, allowing adhesions to form in many instances. All writers, save Keen, agree in urging the importance of an early operation, and even he advises this course, except in those cases in which the shock is very extreme. I think it may be safely stated that with very few exceptions the earlier the operation is done, the better will the outcome be.

We all heard Dr. C. H. Mayo, only this week, in discussing a case of widespread diffuse peritonitis following perforation of a gangrenous appendix on which he was operating, say that in all cases of peritonitis due to perforation we have about six hours' limit in which to operate; after that time, the mortality is extremely high. A review of many statistics shows that this is emphatically true of typhoid perforation.

Harte says that "the early recognition of the lesion, and dealing with it as rapidly as possible in order that as little time as possible may lapse from the time of perforation until the operation has been performed, is of the first importance."

Stewart says, "The operation should be done at the earliest possible period."

THE OPERATION

The incision should be made through the right rectus muscle; since, as Haggard has shown, 95.5 per cent of perforations occur in the last three feet of the ileum, and Harte says that 73 per cent are found in the last twelve inches of the ileum. The latter writer, after careful research through the reported cases, says that when the incision was made in the median line the mortality was 78.12 per cent, and when through the right rectus muscle, the mortality was 68.37 per cent. In exceptional cases the perforation may occur at any portion of the alimentary canal. In one of the cases reported in this paper, the perforation occurred in a Meckel's diverticulum attached to the transverse colon. But the cæcum is our landmark

and from this point we can readily reach every other portion of the bowel.

Another point to be borne in mind is the fact that in sixteen per cent of the cases the perforations are multiple. In one of the writer's cases there was one complete perforation, and four others nearly through.

Upon opening the abdomen, the presence of free fluid and other evidences of peritonitis will usually be found, indicating the existence of a perforation. In a few cases, peritonitis without perforation has been found, and in other instances rapidly formed adhesions preclude infection of the general peritoneum, even in the presence of a perforation. The distal portion of the ileum with the cæcum and appendix should be quickly and gently examined. After these have been dealt with, the remaining portions of the intestines should be inspected, remembering all the time that the less the manipulation inside of the abdominal cavity, the better the prognosis.

The perforation should be closed with a purse-string suture, or any other intestinal suture with which the surgeon is most familiar, being careful not to make too much tension. The author's preference is the modified Horsley suture, since it can be quickly and safely applied. Do not try to trim the ragged edges of the ulcer; simply invert them. Unfortunately, the perforation is sometimes of such size or so situated in a mass of ulcers that it cannot be safely inverted, in which case the perforation should be sutured to the abdominal incision, making a fæcal fistula. Escher advises that this be done in every instance, but making a fæcal fistula does not seem to be the best thing to do in every case.

The rule with many surgeons has been to flush thoroughly the abdominal cavity with salt solution. Harte says the best results have been where the peritoneum was vigorously flushed with salt solution, then with equal parts of salt solution and hydrogen peroxide, followed in turn by the normal salt solution.

I think that possibly there is some question about the propriety of always flushing the abdominal cavity. If we feel that there is much fluid fæces in the cavity, unquestionably the flushing should be done, but if the fluid present consists principally of the serum poured out by the peritoneum to protect itself, then we had better not wash it away, but depend on drainage and absorption to deal with the effusion.

Drainage should always be employed. A gauze wick in a rubber or glass tube is preferable and should reach well into the pelvis, and one should perhaps be placed in the kidney fossa, on the right side particularly. It is well to put the patient to bed in the exaggerated Fowler position, in order that the fluids may drain into the pelvis.

In every case of operation for typhoid perforation, rapidity and gentleness are absolutely essential to success.

ANÆSTHESIA

Many of the first operations were done under local anæsthesia, and some men, notably Cushing and Hayes, prefer it even to-day; but the majority of the surgeons now use ether, which, in my judgment, is by far the best anæsthetic; since, while the abdomen may be opened under the use of cocaine, yet the retraction of the incision edges and the manipulation of the intestines are painful, besides the mental perturbation of the patient from knowing that he is having a serious operation done. All these more than offset the disadvantages of general anæsthesia.

TIME OF PERFORATION

It is important to know when to look for perforation in typhoid. Our medical men need to be trained along these lines, just as the surgeon educated them regarding appendicitis. The fact, already stated, that more than fifteen thousand die annually in the United States of typhoid perforation should put every practitioner on the alert. Typhoid cases, whether walking or bed-ridden, whether mild or severe, should be watched with an eye to the possibility of perforation until convalescence is thoroughly established. As a matter of fact, most of the cases of perforation occur during the third week. Stewart says that perforation usually occurs during the third, fourth, or fifth week, but it may happen at any stage of the disease. Woolsey's average of seventeen cases was twenty-seven days. Courtney says that the majority of cases are seen during the second or third weeks; it may, however, occur

at any time during the latter part of the disease and convalescence; it is not infrequently seen during a relapse.

In one of the author's cases, perforation occurred ten days after the subsidence of fever.

SYMPTOMS

I think our ideas about the symptoms indicating perforation have been misleading. Osler says that "the time-honored picture of perforation of Hippocratic facies, the feeble-running pulse, profuse sweat, distended and motionless abdomen, must be erased as the picture of perforation, but of peritonitis, or, still better, a rough draft of death."

Temperature. Perhaps we have been more in error regarding the presence af subnormal temperature than anything else. Subnormal temperature is rarely present, and when observed, it is usually a late symptom, indicating a profound toxæmia. An early subnormal temperature is far more characteristic of hemorrhage than perforation. Following perforation, the temperature quickly begins to rise, which is one sign of the ever-present peritonitis.

Pain. The most significant symptom of perforation, or rather of the quickly occurring peritonitis, is pain. It is universally present. Woolsey noted it in sixteen out of seventeen cases, and the exception proved to be one in which perforation was surrounded by adhesions making a localized pocket. True, a delirious or moribund patient may make no complaint of pain, even though it be present; it is also true that other conditions than peritonitis following perforation may cause pain, but a sudden pain occurring in the lower half of the abdomen, especially on the right side, after the first ten days of typhoid fever, is very significant of perforation, and would justify an exploratory incision; particularly is this true if rigidity and tenderness quickly supervened. While the pain is usually in the cæcal region, it may be at a point widely divergent from the lesion; for instance, in the epigastrium, or, as in a case reported by Wilson, in the glans penis. The nurse should be taught the significance of abdominal pain in typhoid fever, so that she may notify the physician at once.

Rigidity. The next most important symptom is rigidity of the abdominal muscles, especially if occurring on the right side. It may be present all over the abdomen in advanced cases.

Tenderness. This, like rigidity, is a symptom of the rapidly spreading peritonitis.

Tympany. Tympany is usually slight at first, since enough gas does not quickly escape through the perforation to produce much distention, but it is a prominent symptom later.

Hepatic Dullness. Theoretically, hepatic dullness should appear, but it rarely does till late, because of the small amount of gas in the abdominal cavity. However, Elsberg puts considerable stress on the diminution of liver dullness, and says it is a valuable diagnostic aid, being present in fourteen out of fifteen cases.

Leucocytosis. Much stress has been laid upon the value of leucocyte count, but, judging from the many observations that have been made, it is not of very great value, since it may or may not be present, and to wait to make repeated counts would be bad surgery. In doubtful cases, leucocytosis would be an aid in determining the presence of a perforation, but its absence should not be allowed to count against perforation.

Vomiting. Vomiting is occasionally present in the beginning, and is significant of the shock and peritoneal infection. Woolsey found it as an initial symptom in four of his seventeen cases.

CASES

The author has operated on only two cases of typhoid perforation, a brief synopsis of which is appended.

CASE I. *Typical case of typhoid; perforation on the thirtieth day; extensive peritonitis; operation; death from acute dilatation of stomach.*

G. H., male, single, aged 22, by occupation a railway conductor, was in St. Leo's Hospital, Greensboro, North Carolina, and under the care of Dr. E. A. Ledbetter. The records show that his case was a typical one of typhoid fever, with every symptom present, including the characteristic eruption.

On the twenty-ninth day of the attack, the afternoon temperature was 100.80°. On the thirtieth day the temperature at 6 A. M. was 98.0°; at 9 A. M. it was 98.40°, and pulse 70. At this time he partook of chicken broth; immediately afterward he complained of severe pain in the abdomen. The nurse notified the interne, Dr. J. W. Tankersley, a student of Keen, who made a diagnosis of intestinal perforation. At 10.30 A. M. the record shows: temperature 97.6°, pulse 60, respiration 20. Rigidity, tenderness, and tympany quickly devel-

oped. At 12 M. the temperature was 99.0°, pulse 110. Dr. Ledbetter was early called, and Dr. A. B. Wilson saw the patient in consultation. After some unavoidable delay, the writer joined the consultation. All concurring in the diagnosis, the abdomen was opened under ether anæsthesia at 2:30 P. M., five hours after the perforation.

Evidences of widespread diffuse peritonitis were present. The cavity contained a large quantity of serum and fluid fæces. The distal end of the ileum for about three feet was thick, boggy, and had a sausage-like feel. Twelve inches from the cæcum there was found a tiny perforation measuring perhaps two lines in diameter, situated in a mass of ulcers one and a half by two and a half inches in diameter. Within the next two feet of the ileum there were four other ulcers nearly through. A noticeable feature in these latter was the fact that the peritoneum seemed to be eroded and the ulcers just ready to give way. The threatened perforations were carefully inverted and sutured, but the complete perforation could not be inverted, because of its situation in the mass of ulcers, therefore the bowel was attached to the edges of the incision, making an enterostomy. The cavity was carefully flushed out with salt solution; a split-tube drainage was introduced into the pelvis and one into the right kidney fossa.

This patient gave hope of recovery. The pain, rigidity, tenderness, and tympany promptly subsided. The intestine thoroughly emptied itself through the fistula, and the drainage worked well, but the patient developed one of those unfortunate complications so little understood and so often fatal; namely, acute dilatation of the stomach. As often as the constantly distending stomach was washed out, it would fill again with large quantities of greenish fluid. This continued to the last, and was, I believe, the immediate cause of the patient's death, which occurred on the ninth day, two hundred hours after the operation. This patient certainly did not die from intestinal toxæmia, since the fistula thoroughly emptied the bowel; nor did he die from peritonitis, because the evidences of this complication were rapidly subsiding, otherwise he would have died much earlier. The kidneys performed their function well, and the patient was courageous to the last.

Woolsey says that fifty-two hours was the longest time that any fatal case at the Presbyterian Hospital survived the operation.

Within fifteen hours of the time I operated on the case just reported, I was called by Dr. J. W. Peacock of Thomasville, North Carolina, to see a case giving the following history:

CASE 2. *Typhoid fever; perforation of Meckel's diverticulum; peritonitis; operation; recovery.*

J. C. S., single, aged 25, by occupation a merchant, had typhoid fever with the characteristic eruption, the temperature subsiding to normal on the twenty-first day. For six days thereafter he was kept on liquid diet, and then on soft diet. He had been walking around his room for three or four days, feeling quite well, when, ten days after the fever had disappeared, he was awakened shortly after midnight by a severe abdominal pain located about his umbilicus. Dr. Peacock was summoned at once, and diagnosed the case as perforation of the intestine. He telephoned to me, and with two members of my staff and a nurse I reached Thomasville at 8 A. M., July 27, 1906. The man was living in a miserably dirty cabin. There was not a woman around the premises, and everything was in the greatest confusion. My assistants quickly got things ready and the patient was etherized. The abdomen was opened through the right rectus muscle. The cavity was full of sero-pus. I quickly inspected the ileum, cæcum, and appendix, but found nothing abnormal, except a widespread peritonitis. Passing my hand up along the colon, I found a Meckel's diverticulum located within the folds of the greater omentum and springing from the proximal end of the transverse colon. The diverticulum was about the size of a hen's egg. An ulcer within its lumen had perforated, precipitating the patient into the throes of fulminate peritonitis. The cavity of the diverticulum did not communicate with the intestine. The offending structure was ligated and removed, the abdominal cavity washed out, and drainage introduced, as in the first case. The incision was closed in part, and the man put to bed in the exaggerated Fowler position. Rapid and uninterrupted recovery.

PRIMARY OPERATIONS FOR OBSTETRICAL DEBILITY[1]

The Delivery of Debilitated Women by Operative Methods, Intrapelvic or Abdominal, before Labor, in the Absence of Mechanical Impediments and as a Method of Choice

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THE most important part of the management of labor is the guidance of the natural process into its best channels, and we should supplement it by an artificial delivery only when, in spite of our best guidance, the natural process is failing to give the best results; but progress in obstetrics has consisted, from the earliest times to the present day, in continuous discoveries of new methods by which the minor aberrations of nature can be prevented, and in the application of artificial methods to an ever-widening field of comparative failures of the natural powers, in coincidence with the progressive decrease in the dangers of operative work, which has resulted from its greater frequency and improved technique.

There are two classes of failure in labor: 1. Those in which the bony or other mechanical obstacles are too great to be overcome by the normal expulsive powers; and 2. Those in which the expulsive powers are too weak to overcome a normal degree of resistance. The profession has for some years realized that cases of the first class are best treated by operative delivery at the beginning of labor. The theorem of this paper is the statement that the same principle should be extended to the treatment of a very few of the worst cases of the second class.

In order to avoid confusion and to insure comprehension of the subject, it is necessary to state at the start, and with the utmost emphasis possible, that nothing that is said herein is intended to apply in any degree to the management of strong women with bony deformities or other abnormally high mechanical resistances. In order to avoid confusion, these cases should be excluded from the mind of the reader in each successive phase of the argument.

It is intended to deal only with an exceedingly small class of overcivilized women in whom the natural powers of withstanding pain and muscular fatigue are abnormally deficient.

Obstetrics has always been an exceedingly conservative branch of medicine. It is only within the last few years that it has been perceptibly affected by the progressive movement that has for many years influenced the other branches of practice. It differs from the other branches of medicine, in that it is concerned with the management of natural rather than of pathological processes, and its thought is still dominated to a surprising degree by a reverence for the "natural process," which has been elevated into a fetish or superstition. Were we as well trained in comparative zoölogy as in our detailed knowledge of human labor, we should escape the merely superstitious part of this reverence for the "natural process," and be prepared to take a more completely rational view of its phenomena.

Fifty years ago (1), the burning question of the day was, whether the sufferings of labor might properly and wisely be lessened by the administration of ether or chloroform; and while certain progressives urged the use of these drugs, it is interesting to see, on reading the literature of the period, that for a time the majority of the profession upheld a counterposition, which may be paraphrased to the effect that, since an all-wise and inscrutable Creator had decreed for his own mysterious purposes that women should suffer in labor, it was impious and sacrilegious for mere man to interfere with his plans, and that it could hardly fail to bring retribution, both on those who employed these mediums and on those for whom they were employed. It is interesting to observe that the persons who made these objections to the obstetrical use of ether saw no objection to its use in surgery, the whole trend of the discussion being that labor was a natural process, ordained by the Creator, and one not to be tampered with by man.

[1] Read before the Chicago Gynecological Society, December 21, 1906.

This theological opposition to advance in obstetrics soon lost its hold upon the professional mind; yet it was no sooner abandoned than the same idea crops up again, this time under the guise of a scientific or evolutionary doctrine. If we turn to the practice of the succeeding generation (2), we find anæsthesia adopted and the theological doctrine of impiety abandoned, but the conservatism of the day is arrayed against another point of progress, upon a different yet fundamentally similar basis. The progressives of that day are urging the more frequent use of forceps, while the conservatives oppose them with the obvious truth that labor is a natural process, the product of the great natural laws of evolution; and with the somewhat less obvious, but to their minds conclusive, corollary, that so long as the natural process persists, it is therefore always better than the efforts of art.

This generation has certainly learned from evolutionary study that nature's processes are often wasteful and indirect, and, on an individual point, by no means always the best or most efficient that could have been devised, had that point only been under modification in the development of the race. The thinking world is to-day prepared to consider without prejudice and without a feeling of impiety the question of whether labor in the *genus homo* is one of nature's successes, or whether it may not rather be regarded as one of the unfortunate compromises to which development is sometimes driven; and it may be instructive to look for a moment at the parturition of women from the standpoint of comparative zoölogy (3).

The three prime factors in parturition are: 1. The dimensions and shape of the bony canal, together with the strength and elasticity of the resisting soft parts; 2. The shape and size of the fœtus; and 3. The power of the propelling engine, or uterus.

That which first and most saliently attracts the eye of one familiar with human obstetrics as he turns to a text-book of veterinary obstetrics is the ease and amplitude of the pelvis. The vertebral column is straight, there is no promontory of the sacrum, the axis of the pelvis is straight, the inlet is oval and ample, the pelvic bones are in contact with each other only at the brim, and the greater part of the pelvic canal is ligamentous and capable of expansion in labor. Of this, Fleming, the author of the leading text-book on veterinary surgery in the English language, says, "It is a fact of daily observation that the pelvis permanently widens in animals which have had young frequently"; and again, of the late ossification of the symphysis which occurs in some animals, he says that in young animals it "allows the diameters of the pelvic cavity to be increased during parturition, and [this] accounts for the rarity of difficult births."

As we turn to the soft parts, we are at once impressed with the simplicity of their arrangement, and with the relative absence of the fasciæ and muscles which we have been accustomed to weigh as opposing forces. The reason for this is obvious: the inlet of the pelvis lies above the level of the abdominal cavity and its axis inclines slightly upward; the soft parts, consequently, never bear the weight of the abdominal viscera, are only designed to fill in the ample space between the bones, and are consequently so distensible as to be capable of rapid dilatation whenever pressure is put upon them at any time during pregnancy, and even in the non-pregnant animal. Yet Fleming says, "Under normal conditions, it may be said that the pelvis itself does not offer any obstacle to the passage of the fœtus, and that it is the soft parts alone which oppose its exit."

Turning to the second factor — the fœtus — we find that in veterinary obstetrics the head of the fœtus is too small to play any part in labor; the thoroughly compressible thorax is the only portion of the fœtus which is of a size to meet any essential resistance, even from the soft parts, and, owing to the straight and simple form of the pelvis, there is no rotation, no mechanism of labor as we know it.

The conditions of labor in veterinary obstetrics are thus so easy that the uterus itself is not the great driving-engine of the human female, but is, even in the powerful uniparous quadrupeds, a comparatively thin-walled bifid organ, not unlike a pair of hypertrophied Fallopian tubes, in the slightly thickened junction of which gestation takes place.

Mark the results. Even with this comparatively feeble driving-power the duration of labor is surprisingly short. Fleming says, "In the

mare it [the whole process of labor] is ordinarily accomplished in about ten minutes, sometimes in five, though it may extend to a quarter or half an hour — rarely more." In the "cow, which has the most difficult parturition of any of the domestic animals, it is, on the average, one to two hours; sometimes only fifteen minutes." With the "sheep the period is also brief, being about fifteen minutes." With pluriparous animals, "there is ordinarily a period of ten or fifteen minutes, or even more, between each birth; not infrequently the sow will bring forth ten young ones within the course of an hour." The only delays met with in veterinary obstetrics are those due to malpresentation and monstrosities.

Compare the time of the processes in these statements, made by an authority, with the known duration and difficulties of labor in the human female.

Labor is, and throughout evolutionary time has been, an established success in the quadrupeds. When, however, one group of them were forced by the exigencies of their life to assume the erect posture, the laws of development were face to face with an entirely new problem. The necessity of sustaining and moreover balancing the entire weight of the whole trunk upon the pelvic girdle compelled the development of newly massive and rigid structures with close joints and correspondingly powerful ligaments and muscles. With the assumption of the lumbar curve the sacral vertebræ were forced into the pelvis, while the strain of the sacrosciatic ligaments drew the lower end of the sacrum forward and developed the ischial spines and the curved axis of the rigid pelvis as we know it. The soft parts were now obliged to sustain the thrust of the abdominal viscera, and a group of retaining-muscles, unknown to the quadrupeds, was accordingly developed. The head of the fœtus increased in size and ossification until it became the determining factor on the fœtal side of labor, and also until its passage through the cordate brim and curved axis became possible only by the complicated movements of flexion and rotation which characterize human parturition, and, in the whole scale of animated nature, human parturition alone. With the increasing difficulties of labor the uterus successively developed in structure and power until it has become an engine of such force as is unknown in any other animal.

A comparative zoölogist familiar with labor in the other animals, but confronted for the first time with a hypothetical human female, would probably declare that parturition could not take place at all in the face of such difficulties. The development of these difficulties has, in fact, made human parturition phenomenally hard, but the development was necessary for the supremacy of the race. Without these bony and muscular modifications, primitive woman would have been unable to escape her enemies, and prolapsus and inversion would have been the inevitable result of parturition.

The whole process of the development of the human female has been, throughout, a struggle between the conditions which insure her health and efficiency in the non-pregnant condition, and the opposed conditions which make childbirth possible. The result has been a compromise. Race efficiency and race survival have been preserved, but at the sacrifice of ease of labor in all individuals, and of its safety in the weaker individuals.

Nature is always careless of the individual. Modern medicine fulfills the function of what is known in evolutionary language as the supervention of an easier environment, which permits the preservation of the weaker individuals, often to the great benefit of the race (4).

In the course of the development of this compromise between the necessities of ordinary efficiency and those of parturition, the majority of individuals have reached a state in which a fair degree of both benefits has been obtained, but, under the ever-present law of variation, there are many individuals whose development has erred in one direction or the other. At one end of the line are those individuals who have of late been attracting so much attention from our orthopædic brethren, in whom the pelvic joints are not sufficiently fixed — the subjects of sacroiliac subluxations — and those in whom the soft parts are insufficiently rigid to maintain the ordinary balance of the pelvic organs — the subjects of retroversion and prolapse. At the other end of the line are those in whom the forces opposed to parturition are overdeveloped, or in whom the driving forces are too weak. It

is with this latter class that we are especially concerned.

Much may be learned of many other obstetric problems by a study of our development from the quadrupeds; but the lesson which I think we should here draw from comparative physiology is, that labor in the human female has been developed onto a plane of such extreme vital effort against overdeveloped obstacles as to place favorable natural labor beyond the powers of the obstetrically weaker individuals.

Human obstetrics has, in point of fact, long recognized the principle that the weaker individuals should be artificially delivered before serious exhaustion sets in. The only practical question is, whether we have or have not carried this principle far enough to attain the best results in those who stand at the lower end of the class of weaker individuals. I think that we may yet advance along this line. There is, in fact, at this lower end of the series, a class of women which it is difficult to define academically, but which is well known to all experienced practitioners. These are women upon whom we, as surgeons, do not hesitate to perform any needed operation, counting with certainty upon a fairly good convalescence, but whose pregnancies and labors, we nevertheless, as obstetricians, instinctively dread. They are neurasthenic women, who bear pain badly, of poor muscles, including of course the heart muscle, and usually of deficient lung capacity. They have, ordinarily, ill-developed eliminative systems, and are consequently unable to dispose easily of the products of any unusual excess of metabolism. They are delicate women of deficient muscular and nervous power, whom even the most ordinary observer would know to be unfitted for the muscular exertions and exposures of ordinary out-of-door life, and who, in point of fact, never subject themselves to such — the women who faint, or become otherwise unable to keep up when subjected to more than ordinary muscular exertion, or, rather, to more than that to which they are ordinarily accustomed. They are not confined to the more luxurious classes, though they are less common in the working classes, since the latter more often fail to reach maturity and maternity among their surroundings.

These women are not especially liable to disease, and they convalesce from operation sufficiently well. Why are they unfitted for labor? The answer to this question is to be found in an inquiry into the physiological causes of exhaustion in labor.

I well remember a case which I saw in consultation about fifteen years ago, in which a young and healthy woman had been allowed to drag along through a powerful labor until her exhaustion was so great that the attendant became alarmed. On my arrival, the head was on the perineum, and could evidently have been expelled by a few powerful efforts, but the uterine contractions had died away, and the mother was so exhausted that she was unable to use her voluntary muscles. The delivery was made with forceps, with a single traction, but the patient failed, and died a few minutes after delivery, without apparent reason, and a careful and complete autopsy by an experienced pathologist failed to show any cause of death. This was apparently death from exhaustion in labor uncomplicated by any pathological conditions. Such deaths are now rare, but the lesser degrees of exhaustion are far from rare, and from the study of the more extreme cases we may gain a better knowledge of the lesser.

Immediately after this experience I made a careful effort to find in physiological literature any researches bearing upon the cause of such deaths, and in preparation for this paper have repeated this search, but without conclusive results, the nearest thing which I have found being numerous studies of the production of general fatigue from prolonged muscular exertion, and some *dicta* about nervous exhaustion from pain *per se*. I am forced to conclude that physiological research is as yet far from complete on either of these points, and especially so on nervous exhaustion, but the provisional results reached are as follows (5):[1]

General exhaustion from muscular exertion is due: 1. In part to the effect of a mild toxæmia, produced by the presence in the system of an excessive amount of muscular activity; 2. In part to the diminished activity of the heart, due to such toxæmia; and 3. In part, of course, to direct muscular fatigue of the heart muscle itself. These conclusions are the result

[1] It is to be noted that all these effects will be more rapidly produced if there is oxygenation due to poor lung capacity or by any inefficiency of the eliminative system.

of experimental work by many observers, and are fairly conclusively established.

Unfortunately, the obvious humane objection to the experimental study of the effects of extreme pain and fright has necessarily prevented the direct physiological study of general nervous exhaustion *per se*, with the exception of some early experiments, the publication of which excited universal indignation. The little that we know of it is therefore merely the result of deduction from the general knowledge of physiologists, and is to the effect that the endurance of pain through certain nerves in some way lowers nervous activity throughout the body. General exhaustion from pain is probably the result of diminished activity of all the organs, and this is perhaps due to a toxæmia produced by defective action of the digestive and eliminative systems, due to the ineffective condition of the nerves which guide and inspire them. Individuals of poor nervous organization will, of course, experience such exhaustion sooner than others.

These theoretical results of experimental study are supported by the practical experience obtained in training athletes. Training is a process which aims at putting an athlete into a condition of the highest possible resistance against exhaustion from muscular and nervous effort. It has been elaborated in the course of many generations of practical experience, and is, in brief, a process of carefully eliminating from the system all excess of metabolic products by careful dieting, sweating, rubbing, etc., and then, by a continuance of the same hygiene, enabling and training the organs of elimination to dispose rapidly and effectually of the larger amounts of these substances, produced by carefully graduated increase of the daily exercise. In this it will be seen that long established practical experience exactly confirms the results of experimental research.

Modern surgery involves exceedingly slight interference with the vital processes; it places very little strain on the eliminative organs, and almost none on the heart and lungs; but labor is the exposure of an untrained individual to the effects of long-continued muscular effort and pain. Most women endure it surprisingly well; but it is very evident why these weaker women of whom I am speaking endure operation well and labor badly, when we consider that they are by definition women who combine inferior musculatures, hearts untrained by exercise, deficient lung capacity, and untrained and usually naturally inferior eliminative systems, with a neurasthenic lack of nervous force (6).

To sum up the theoretical conclusions to which this argument has led: Comparative zoölogy shows us that natural labor in the human female is not the perfect process which ancient prejudice considered it, but is an evolutionary compromise, which was necessary for the supremacy of the race, but which bears very hardly on the weaker individual. A reference to the direct physiology of labor shows that individuals who combine weak musculature with poor eliminative, circulatory, and respiratory systems and deficient nervous force are peculiarly ill adapted to the endurance of natural labor, on account of the ultraphysiological strain which it imposes upon these organs, and it is a fair conclusion that for them natural labor is not the best method of delivery. The question which immediately arises, i. e., whether the obstetric art has yet come to a point at which we can recommend some other method of delivery as being safer and otherwise preferable in these cases, is a practical one, which can be answered only by experience, and these theoretical deductions are worth nothing unless they are borne out by the results of practical observation.

If any one will select from the records of his past practice, as I have done, all the women whose labors have been characterized by an inability to effect any essential dilatation of the cervix before the signs of impending exhaustion made him wish that it were possible to deliver them immediately, he will find, I think, that all these women showed, in varying degree, the varying factors of incapacity for labor which I have enumerated, but he will not find that all the women who present these characteristics do badly in labor; he will find, on the contrary, that some women who would be placed in this class before pregnancy are in fact relieved of their neurasthenic symptoms, and gain in muscular force and strength throughout pregnancy, while others, apparently no worse equipped at the start, pursue the opposite course. We cannot as yet altogether explain this difference,

but, after more than ten years of careful observation of considerable numbers of women with especial reference to this problem, I have come to the conclusion that by careful observation of the course of pregnancy in such obstetrically ill-equipped women we may find an almost infallible guide to the management of their labors.

We must remember that the strains of labor are, in many respects, but an exaggeration of those of pregnancy. The increasing development of the child during pregnancy normally puts a constant and increasing strain upon the muscular, circulatory, and eliminative systems of the woman, and during the latter half of its development embarrasses her pulmonary system. When this gradual increase of load is met by nature with a gradual increase of the efficiency of the organs, it may safely be argued that the woman is successfully "training" herself for the greater exertions incident to labor; when, as is so often the case, the nervous symptoms of an underdeveloped woman decrease during pregnancy, it may safely be argued that she will also be fit for labor, from the nervous standpoint. Many previously ill-equipped women are developed by pregnancy and do well in labor; such cases have usually had underdeveloped genital organs, and may even be permanently bettered by the uterine and general development involved in successful pregnancy. It is when the converse is true that we should be afraid of labor.

When a previously ill-equipped woman becomes throughout pregnancy less and less disposed to exertion, and progressively less able to endure it; when, in spite of appropriate care, her digestive and eliminative systems are less active; when an increasing deficiency of the circulatory system is shown by the character of the heart-beat and by the appearance of œdema in the lower extremities; and when, in addition, the nervous symptoms of pregnancy are unduly and increasingly marked,—the woman will surely have a more or less ineffective and unfavorable labor. Whether or not the character of her labor will be so markedly poor as to place her in the class for whom I am recommending operative interference at the beginning of labor, will be determined largely by the care taken of her pregnancy, and cannot, of course, be determined by the application of any general rule. Each case must be studied individually, and the relative weights of each factor in the case must be carefully estimated before a decision is reached; but I have become convinced that an experienced man can by such careful study distinguish, in advance, with a fair degree of certainty, the cases for whom natural labor is disadvantageous.

Not all these cases will fail to deliver themselves if left unassisted. Whether or not spontaneous delivery will eventually take place, even under their comparatively feeble expulsive powers, will depend upon the degree of resistance offered by their pelves and soft parts, and their treatment must be determined by the resistances present.

Some of these women have bony deformities, or other plain indications for an operative delivery. These may be set aside from our present inquiry.

In others, the pelves will be so ample and the soft parts so very lax, that, even with feeble expulsive power, they are likely to deliver themselves. The consideration of these we will postpone for the moment.

There remain for our present consideration women, originally ill equipped, who have deteriorated during pregnancy, either in spite of exact care or for the want of it, and who present average resistances in the pelvis and in the degree of rigidity of the soft parts. With even average resistances these women will rarely deliver themselves, and if they are left to natural labor, the pains will usually be irregular, painful, and have but little effect on the os. Impending exhaustion will be apparent before any descent of the head has been made, and usually before any essential dilatation of the os has been effected. If, then, the case be etherized and the hand passed into the vagina for dilatation of the os, all the conditions will usually be unfavorable for operation; the cervix will be found to be thick, spasmodically contracted, and brittle or friable; i. e., exceedingly likely to tear unless the greatest care is used. When the hand is passed into the uterus, its walls will often be found to be in tonic rigidity, and in most cases applied to the child in one or more zones of hour-glass contraction. If a forceps

operation be done, the head will advance readily under the first tractions, but this progress will shortly fail, and further advance will demand more and more powerful efforts, while the head will show a tendency to recede during the intervals between tractions. If forceps fail and version be resorted to, either primarily or after their failure, both the turning and the extraction will be rendered difficult by the rigid and resilient soft parts. Every obstetrician of experience is familiar with the difficulties and dangers encountered in this class of operating.

These assertions, too, are easily referable to a review of one's past experience in general. If any one will again look over the records of all those cases in his past practice in which he has been obliged to interfere with the head high because the pains had failed to dilate the os and the woman was becoming exhausted, he will find, I think, that the operation has been almost invariably hard, and usually attended by considerable danger and subsequent exhaustion on the part of both mother and child.

I wish to quote two cases illustrative of the different methods of management of such patients.

CASE 1. Mrs. T., primipara, 35 years old. Had never been an invalid, but had never been well. Exceedingly thin; always anæmic. Never well enough to indulge in any out-of-door sports. Had had two abdominal operations; one for the removal of a cystic ovary, and one for a so-called floating kidney. Was described in a letter from the very eminent surgeon who had attended her "as a creaking door" and as never having strength enough to be well. He said of her convalescence from the first operation, "She made a good recovery, and did reasonably well after the operation," and of the second operation, "From this operation she also made a satisfactory recovery."

She was prostrated by a considerable but not an alarming degree of the vomiting of pregnancy throughout the first five months, and when this subsided, was so weak, languid, and nervous as to pass the greater part of every day in bed or upon a lounge. It was with the greatest difficulty that she was induced to leave the house at all. The urine showed decreased metabolism, but was otherwise normal. When taken in labor, she suffered, from the first, extreme pain, which, at the end of three hours, became intense and never wholly absent. But little if any dilatation of the os was accomplished. At the end of a very few hours it was evident that the prospect of her ultimately delivering herself was very small, and her sufferings were so intense that, early in labor as it was, she was etherized and delivered. The cervix was very rigid, and was dilated with the utmost difficulty, retaining its resiliency to the end and recontracting whenever relieved from pressure. The head presented O. D. P. behind the membranes; the hand, introduced into the uterus for the purpose of rotating the child to an anterior position, found the uterus in tonic contraction and a constriction ring about the middle of the child. After much difficulty and by pressure on the shoulders, the child was rotated until the head lay in an anterior position; forceps were applied, and a nine-pound living child extracted after a long struggle. The child was much bruised, but was resuscitated without unusual difficulty. The cervix and perineum were both torn extensively. The perineum was repaired successfully. Convalescence was uninterrupted, but was long and unsatisfactory, it being nearly a year before the patient was as well as at the time of her marriage. I was subsequently obliged to perform a third abdominal operation upon her for the relief of an acutely threatening appendix. Her convalescence from this was thoroughly satisfactory, though, of course, somewhat slower than would have been expected from a robust woman.

My management of this case is thoroughly open to criticism. Upon the one hand, it may be said that if she had been allowed to drag through a lengthy first stage, and not delivered until definite physical signs of exhaustion were present, the head might have advanced so far that the forceps operation would have been less difficult. This is possible; but I doubt if her strength would have lasted long enough to effect any essential advance, while every hour of delay would certainly have decreased the chances of the child, and the performance of a difficult operation in the presence of a rapid pulse would certainly not have been any more favorable to the mother than when it was undertaken before absolute exhaustion was present. My own criticism is, that this case should have been delivered by the Cæsarean section, from which I am confident that she would have convalesced as after her three other abdominal operations, but I had not at this time reached the point of being willing to recommend the section in the absence of bony obstruction.

In contrast to this case I wish to quote a second.

CASE 2. Mrs. B., primipara, 29 years old. Previous to marriage, a teacher in a normal school. A woman of considerable mental brilliancy, but of extreme nervous instability. Pale, anæmic, and of very dry, hard skin. Though always a semi-invalid, had been able to fulfill her duties with but few intermissions, though at the cost of great fatigue, and only under the pressure of absolute necessity. After marriage, and relief from her duties, she rapidly became hysterical, and suffered from every variety of nervous ill. Was dyspeptic, insomnic,

and obliged to stay in bed a portion of almost every day.

She was brought to me early in her first pregnancy, for an opinion as to whether she was physically capable of going through pregnancy. After seeing and talking with her, I was confident that she could be carried through pregnancy, but that her labor would be very trying. On examination I was much relieved to detect a moderately contracted justominor pelvis, and advised that she should be given every care during pregnancy, that the development of the child should be watched, and that Cæsarean section would be necessary unless the child were very small. I saw her repeatedly during pregnancy. She did fairly well physically, but the nervous instability reached an extreme point, and, as the child was evidently large, I advised that she be etherized at term and the method of delivery decided then. The membranes broke the evening before the appointed date, and she was etherized five hours later. Feeling uncertain of the size of the child, under ether I very easily dilated the os, made a tentative application of forceps, and satisfied myself that it was too large for easy delivery. I then did Cæsarean section, the child proving to be a ten-pounder. Mother and child did well, convalescence being uninterrupted, except by extreme nervousness, and as prompt as could be expected in such a patient. Her subsequent health was about that previous to confinement.

Five years later she was sent to me, again pregnant. She was in somewhat worse general condition, and the course of her pregnancy was somewhat less favorable physically and somewhat better nervously than on the first occasion. She was again delivered by Cæsarean section, and again did uninterruptedly well. Under constant care, and with a very easy life, she has maintained about as good a degree of health as could be expected.

I have so far performed the Cæsarean section only once in the absence of bony deformity, but I now believe that those ill-equipped women of constitutionally low expulsive power, whose pregnancies are unfavorable, and who present mechanical conditions which are either hard or average, should be subjected to a primary Cæsarean section.[1]

I believe that they should be treated by Cæsarean section because I am one of the many obstetricians whose experience has led them to believe that primary section is a lesser operation than hard, high forceps or version.

The maternal mortality of the late section (that performed after the mother is already exhausted in labor) is as high as it was twenty years ago; the mortality of the early secondary section (that performed after a moderate test of the patient in labor, but before the signs of exhaustion have become apparent) is believed by most obstetricians to be no more than that of hard intrapelvic operations; while the mortality of the primary section (that done at an elected date, or with the advent of the first pains of labor) has of late years been practically *nil* in the experience of all obstetricians.

My own experience with Cæsarean section fully supports these general results. It has been much smaller than it would have been had I not resigned my obstetric service five years ago, at about the time it was becoming frequent. Since that time my sections have been limited to cases in private practice, referred to me for the purpose; but I have done twenty-nine Cæsarean sections, thirteen of them in private practice, without losing mother or child. I attribute my good fortune to the fact that all my cases were operated on before the mother had been exhausted by long labor; while, certainly, the absence of fœtal mortality and the after-condition of the mothers have been in marked contrast to those of any similar series of hard intrapelvic extractions. The superiority of the strictly primary section has also been apparent in my own series of cases.[1]

We have, so far, left unconsidered the remaining moiety of the women we are considering, in whom the resisting forces are so slight that they may fairly be expected to deliver themselves if left to natural labor. While it is, of course, impossible in such matters to draw sharp lines, and each case must be considered on its merits, it may be said without reserve that these women should not be subjected to Cæsarean section.

[1] In these women, some operation is always necessary, and intrapelvic operations are always hard.

[1] In five of my first eighteen cases, fair labor had been present for periods varying from eleven to nineteen hours. In one case, six hours of good labor had been preceded by thirty hours of preliminary pain; nine had less than seven hours of labor; four were operated on in advance of labor. A careful study of my records, made in 1900, at the end of that series, convinced me that the patients' sufferings and my own anxieties during convalescence had been pretty closely proportional to the extent of effective labor that they had suffered before operation, and that the contrast was very great between cases operated upon during labor and those in which it was undertaken before any essential labor had occurred. I then decided that in my future cases I should, so far as possible, operate in advance of labor.

I have since then operated on one case which I saw in consultation after she had had a considerable preliminary stage and six hours of fair labor, but the other ten of my last eleven cases have been recommended a section at the calculated date of term. In eight of them the operation was so performed; in two, labor appeared slightly before the calculated date. The operation was performed as soon after the first pain as possible. In all these ten cases the convalescence has been but little different from that which we should now expect after a ventrosuspension or the removal of a small ovarian.

I am therefore now convinced of the superiority of doing the Cæsarean section either at the calculated date or with the advent of the first pains whenever it is to be done at all, my preference being for the calculated date.

Those in whom the powers of expulsion, on the whole, so far outweigh the resistances that labor may be expected to be very brief should be left to natural labor, or an extraction should be made during labor, if it is needed; but some of these in whom the balance tends the other way will do better after a primary pelvic operation than after even an easy natural labor, since even a fairly easy natural labor will produce a very serious amount of exhaustion in some few of these very feeble women.

I wish to quote two cases in illustration.

CASE 3. Mrs. A. This patient was a very fat, flabby woman of pale and pasty complexion. She belonged to an excessively neurotic family, every other member of which was an invalid. She was a woman of brilliant mind, perhaps a little of a crank, but otherwise possessed, personally, of a very fair nervous system. She had fair muscular endurance, but was always somewhat short of breath on an up grade, and especially during pregnancy. Her urine never showed any essential abnormalities, but she was markedly œdematous during the latter part of every pregnancy. She always suffered from moderately severe vomiting during the first half of pregnancy, and was very languid and tired during the last half. *First Labor:* Placenta prævia at seven and a half months. Delivered by version, in the face of hemorrhage. *Second Labor:* Two years later. Multiparous os and perineum, both slightly lacerated at previous delivery, both soft and dilatable. Ineffective, lingering pains for many hours. Patient nervous and complaining. Pains gradually improved, os slowly dilated, first stage ended after about twelve hours, remainder of labor fairly easy. Convalescence normal, but lingering. Patient unable to nurse, and in debilitated condition for many months. *Third Labor:* Similar, but terminated by forceps as soon as the os was fully dilated. Convalescence about the same. *Fourth Labor:* In this labor she was directed to send for me with the very first pain, and was etherized immediately on my arrival, about an hour later. The os was dilated manually without the slightest difficulty, and as the head was high and free, and the mechanical conditions exceedingly easy, the child was turned and delivered with the most extreme ease. The dilatation occupied about twenty minutes; the version and extraction were estimated at two minutes. The placenta was easily expressed a few minutes later. The rapidity of the convalescence and the prompt return of the patient to her usual condition of fair health was in marked contrast to former occasions, and was surprising to both the patient and myself.

CASE 4. Mrs. H. Patient's mother of good family history, but died in puerperal mania. Father a man of great ability and marked success, but a lifelong dyspeptic and never well. Patient herself dyspeptic, anæmic, and of deficient nervous force. Had always had the habit of lying down two or three hours every day, and suffered from headache if she were unable to do so. Never well enough to take any active exercise or endure the most ordinary exposure to cold and wet. Was generally regarded as a delicate woman, but by these precautions preserved fair health. Was in a condition of rapidly increasing debility throughout pregnancy. I had not then reached the practice of delivering such patients by primary operation, although I realized its advantages. On visiting her in her first labor, after about two hours of trifling pains I was, for her sake, delighted to find that the cord presented, the os admitting one finger freely, the cervix and vagina lax and soft (unusually so for a primipara), the pelvis ample. She was immediately etherized, the cord was pushed back out of the way, the cervix manually dilated without especial difficulty, the child turned and easily delivered. (This case would have been more appropriate for forceps, had I not feared to use them, because the cord was still alongside the head.) I had known this patient for many years, and after her progressive loss of strength in pregnancy had been exceedingly anxious about her labor. Her convalescence was very rapid, and she appeared to be rather improved in health than otherwise during the succeeding year. *Second Labor:* Pregnancy about similar to the first. Head presentation, pains feeble and ineffective, but progress fairly steady, suffering rather extreme, but steadily eased by administration of small amounts of ether. Normal delivery. Second stage of labor less than two hours; total of labor, twelve hours. Patient much exhausted after labor. Did not fully react for about twelve hours. Convalescence very slow, was in exceedingly poor health for more than a year, being obliged to devote herself to a rest cure for several months. The difference of convalescence in these two labors may, of course, have been a coincidence, but I do not think it was. She has since had a third labor, which was at a distance from Boston, and was not under my care, but was conducted in accordance with my previous experience in her case. She was delivered by manual dilatation and forceps on a fixed date before the beginning of labor, and made a fair convalescence, much better than after the second labor. She entered on her fourth pregnancy in a distinctly unsatisfactory nervous condition, but by absolute regulation of her life was brought to labor in a much improved condition over that at the beginning of pregnancy. This labor was very rapid—less than four hours from the time of the first pain until the delivery of the child—and during the first two hours the pains came at intervals of every twenty minutes to half an hour, so that practically the active labor lasted only two hours. Convalescence was satisfactory; better than after the third labor. This case is a very fair illustration of the whole subject of the management of these very feeble women with low resistant forces, and especially of the importance of extreme care in their pregnancies.[1]

I believe that in extremely debilitated women, such as these, the most extreme care of pregnancy is of the first importance, but that when

[1] See Dr Newell's remarks, in discussion, *post*, p. 368.

this has been neglected, or in the very rare cases in which it fails to prevent deterioration during pregnancy, they do better after an operative delivery than after natural labor, the method to be determined by the mechanical conditions present both in the bones and soft parts; those in whom an intrapelvic extraction might be expected to be difficult being subjected to the Cæsarean section, and those in whom it may be counted upon as easy being delivered by an intrapelvic operation.

It will at once be asked if there are not many transitional cases in whom the choice between these operations is difficult. This difficulty may, however, be met by deferring the decision in doubtful cases until after the patient is on the table, prepared for either operation; if, then, after pressing the head into the pelvis from above, and passing the fingers into the cervix, the operator thinks the conditions sufficiently easy to warrant an intrapelvic operation, he should proceed with the dilatation of the cervix, and after its completion apply forceps to the head and draw it gently into the brim, making no attempt at extraction, but simply testing the adaptation between head and pelvis. If he thus convinces himself that the case is within the limits of easy intrapelvic extraction, it will be easy to complete the operation; but if, on the contrary, he encounters unexpected difficulty, I think that he may still safely resort to Cæsarean section. I have resorted to this maneuver many times during my earlier experience with Cæsarean section in doubtful cases. It is to be avoided, if possible, but the information which it gives is of the greatest value, and I do not think that in skilled hands it complicates a subsequent section essentially; i. e., I have not seen more difficult convalescences after this operation than after primary sections. I am sure that it is a far less complication than that of testing the adaptation in these cases by allowing the patient to go into labor before performing the section.[1]

The delivery of a few of these debilitated women by a primary operation of the kind appropriate to the individual case is, I believe, justified by a decreased maternal morbidity and some decrease of fœtal mortality, as well as morbidity, as compared with natural labor in such cases, or extractions undertaken during labor, so long as it involves no increase in maternal mortality. That mortality is not increased and morbidity is lessened by such procedures, can be learned only by experience.

Since an increased mortality would, of course, rule out the primary operation, that point should be discussed first. Such a method as I propose for these few extremely debilitated cases implies the performance of some few primary Cæsarean sections in cases which might otherwise be delivered by intrapelvic operations during labor, but primary Cæsarean sections are lesser operations than the hard extractions which are ordinarily encountered in these cases. It involves a considerable number of primary intrapelvic extractions in cases with all the conditions easy which might have struggled through to a low forceps operation or even a spontaneous delivery, but I have learned from experience that extractions in these easy cases are always easy extractions, and, to a skilled operator, really easy extractions show no mortality to either mother or child.

I believe that primary extractions are always easy in these lax cases when undertaken at the very beginning of labor, and, further, that they are easy because they are undertaken before the uterus has been thrown into contractility by the stimulus of labor. I believe that if any one will look over the records of those cases in his past practice in which some emergency other than eclampsia[2] has forced him to dilate and deliver artificially under easy mechanical conditions, he will find that the operation has always been surprisingly easy, and successful for mother or child.

As I review the fourteen years during which I was in charge of a hospital clinic, which brought me in contact with great numbers of cases, I am confident that this has always been my experience. I believe that the easy dilatation and extraction is due to the fact that in these emergencies the patients are etherized and the operations performed before the cervical and uterine muscles are awakened into

[1] Such gentle tentative use of the forceps by an expert, with the patient on a table, and after full technical preparation of the field of operation, is a wholly different matter from the failure of an actual attempt at delivery with the forceps, more especially if this has been undertaken under the ordinary conditions of labor. This latter, in my opinion, contraindicates a subsequent section, both from the trauma which it necessarily inflicts on the lower uterine segment and from the risk of infection which its performance, under the ordinary conditions, necessarily involves.

[2] In eclampsia, the uterine muscle is always in a rigid state

active contractility by any considerable amount of labor.[1] This is an idea which I was at first unwilling to entertain, but which has strengthened itself in my mind with every new experience, and with every fresh review of my past experience.

It has long been held, for instance, that dilatation and extraction is always easy in placenta prævia, and this is as true where only the edge of the placenta is at the os as it is in cases of central implantation. Indeed, though the lower uterine segment may be weakened by the previous insertion of the placenta, the constriction ring of the cervix is entirely unaffected by it. My experience with the ease of intrapelvic operations undertaken before the onset of labor for other causes has made me believe that the acknowledged ease of extraction in placenta prævia is due to the fact that in these cases the onset of labor is usually announced by hemorrhage and that the operation is undertaken before the uterus is in contraction, rather than to any peculiarity of the tissues in placenta prævia. So, too, in the absence of bony deformity, extraction is always easy in cases in which the cord prolapses or presents at the beginning of labor. This is because, in the absence of bony deformity, this accident occurs, ordinarily, only in cases with lax soft parts, and the extraction is nowadays undertaken imdiately and before the uterus has been thrown into contractility by any considerable amount of labor. This is a new idea, which I was at first unwilling to entertain, but which has strengthened itself in my mind with every review of my experience, and, at all events, I think we may safely feel that in these very easy cases the mortality involved in a primary intrapelvic operation is to-day no larger than that of normal labor.

That the morbidity in these debilitated women is less after primary operation (by whichever method is indicated by the mechanical conditions) than after normal labor is not easily proved, either in writing or to an audience; but I have convinced myself that it is so, after nearly ten years of careful observation of many cases delivered by all methods with this idea constantly in my mind, and have checked my general impression by a recent review of such cases of this nature as I have seen in private practice sufficiently lately to enable me to remember them in connection with their records.

Such cases are never, in the aggregate, very numerous in any one man's practice. That my experience with pregnancy and labor has of late years been limited to consultation practice has perhaps rather increased than diminished their number, since a consultant sees, naturally, those cases of pregnancy about which alarm is felt. On reviewing my record files, I find that I have seen in the last five years twenty-one such cases, delivered in one way or another, as indicated by the extremes I have selected for quotation. A careful review of their records has convinced me anew of the strength of the position, that the earlier these women are delivered, the better their convalescences, the less their subsequent ill health; and I have been rather surprised to find that the frequency of extensive lacerations was very plainly in inverse proportion to the amount of preliminary labor they had undergone.

So indefinite a question as the effect of one or another procedure upon the general condition of patients afterwards is manifestly one which is incapable of a statistical report that would be worth the paper it was written on; but my conclusions have been reached after a very slow and painstaking process during a long experience, and I submit them as such, with a firm belief that in the long run they will be confirmed by a consensus of opinion both that primary operation produces in these feeble women better ultimate results (i. e., no increased mortality and much less subsequent morbidity) than operation undertaken after exhaustion is present, and that exhaustion occurs surprisingly early in this worst-equipped class of women.

When a primary operation of either form has been decided upon, some obstetricians will prefer to undertake it with the advent of the first pains of labor, while some will prefer its performance at a day of election at or about the calculated date of term. This is, I think, a matter of detail which depends upon the choice of the individual attendant, and one which has been so much discussed of late in connection

[1] Always excluding eclampsia, in which the uterus is from the start in a state of contractility

with Cæsarean section as to need no more mention here.

To sum up my conclusions, I believe that when we are confronted with pregnancy in one of these obstetrically worst-equipped individuals — i. e., neurasthenic women, with little nervous endurance or capacity for bearing pain, with poor muscles, heart muscle, and lung capacity, and with ill-trained and usually deficient eliminative systems — we should, during this period, establish a most careful regulation of habits, and should submit them repeatedly to the most extended and careful constitutional examinations, thus endeavoring not only to improve the functions especially deficient in each case, but also to gain a complete knowledge of improvement or loss in condition during the nine months.

The cases which gain during pregnancy may safely be left to the test of labor, as a rule; but even these favorable cases should always be delivered as promptly as is possible with safety, and in advance of any considerable degree of exhaustion. When, on the other hand, we find that an obstetrically weak woman has retrograded to any considerable degree during pregnancy, I think we should consider with the utmost care whether she is not one of the cases for whom the inevitable fatigues of natural labor are worse than the disadvantages of predetermined operative delivery.

If the answer is in the affirmative, I think that, either at the calculated date of term or with the advent of the first pains of labor, she should be carefully prepared, fully etherized, and placed upon the table as for a surgical operation, with abundance of assistance present; that the mechanical conditions should then be carefully determined under ether, by pressing the head into the brim from above, and by estimation of the laxity of the soft parts; that if the conditions are anything but easy, she should be subjected to Cæsarean section; but that if, on the other hand, they are easy, and the section is thought to be unnecessary, the cervix should be dilated, forceps applied, and strictly tentative tractions made; if this procedure develops the existence of more than moderate resistances, section can still be done; if, however, the conditions are still thought to be easy, the child should be extracted by forceps and the mother returned to bed as after any other operation.

I am confident that in the hands of really experienced men such a procedure, so guarded, will be found to be thoroughly safe for both mother and child, and that the better convalescence and less subsequent morbidity of the mothers will, in these extremely ill-equipped cases, be in marked contrast to their condition when they have been delivered after being allowed to exhaust themselves in labor.

Our right to the choice of a predetermined operation at either time, in place of natural labor, rests wholly on our ability to distinguish in advance the cases for whom the exhaustion of natural labor is a greater disadvantage than the inherent disadvantages of all operative work. I can only say for myself, that, after many years of careful study of many cases in which I have not dared to put this principle into full operation, but have adopted the more cautious method of testing my preconceived opinions in labor and carefully noting the result, I have convinced myself that an experienced man can detect such cases in advance with sufficient accuracy to prevent any considerable percentage of unnecessary operating.

I would, however, say further, that I do not believe that any man has any right to accept such a suggestion on the authority of another investigator, nor until he has convinced himself by similar studies of his own experience that be can make this selection with a fair degree of certainty. It will never be proper practice for the unsupported judgment or unaided operating of inexperienced men; but, I repeat, in the class of ill-equipped women whom I have attempted to define, and in the hands of experts (and with strict limitations of predetermined intrapelvic operating to mechanically easy cases), I believe that this principle will make for the greatest good of the greatest number.

I know that I am not alone in holding these views. I know that similar studies are being made by other men. I believe that this topic of the best treatment of constitutionally feeble and debilitated women in labor will be prominent in obstetric discussions in the next few years, and that we shall come to a consensus of opinion upon it; in the mean time, the possibilities of mistake are so many, and the responsibility

involved is so considerable, that few wise general practitioners or embryo experts will care to assume it. I know that others who hold convictions similar to mine, and have not hesitated to put them into practice, have been restrained from expressing them publicly, by the feeling that their indiscriminate use would lead to a vast amount of harm from unnecessary and indiscriminate operating. I think myself that the time has come when this subject is ripe for discussion in societies of specially trained men. I believe that while the awakening of general interest in any such operative subject must inevitably tend for a time to the injury of some patients at the hands of overconfident and ill-equipped practitioners, such harm will be compensated to the greatest good of the greatest number, by a great lessening of human suffering and of maternal morbidity, by some slight decrease of maternal mortality and a great decrease in fœtal mortality, among the patients of many conscientious practitioners, who will gladly avail themselves of any such advance so soon as any considerable number of men, who have become convinced of its worth by practical experience, will place it before them. I believe, too, that the immense force of prejudice among the laity in favor of the natural process, the odium which must inevitably attach to maternal mortality after an operation undertaken in advance of labor, and, more especially, to such an operation undertaken by any practitioner of less than established obstetrical reputation, will, in practice, exercise a strong deterrent effect against any extension of such operating beyond the very narrow limits to which it would naturally be restricted by men of wide experience.

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RETICULUM IN SMALL ROUND-CELL AND LYMPHO SARCOMATA

A Comparative Study

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STATEMENT OF THE PROBLEM

IN the larger handbooks of pathology, and in various treatises dealing with sarcomata, rather definite statements are made concerning the abundance and characteristic arrangement of reticulum in lymphosarcomata. Although it is true that the fact is recognized that reticulum occurs in the other varieties of sarcoma, yet comparatively little significance is attached to this fact. The richness of reticulum in lymphosarcomata is naturally attributed to the fact that these tumors originate only in lymphoid tissue, and lymphoid tissue itself is very rich in reticulum.

The similarity in the histological structure of lympho and round-cell sarcomata is notoriously confusing. One has only to place side by side, under the microscope, specimens of lympho and round-cell sarcomata to appreciate the almost insuperable difficulties of differentiation. Indeed, in a by no means small proportion of cases, differentiation between the two varieties of tumor is impossible without an accompanying clinical history.

The thought is not far distant, that if the lymphosarcomata are richer in reticulum than are the round-cell tumors, this greater richness will be a positive aid in differentiating between these two types of tumor. *The problem therefore is, to get rid of the cells which rest on and obscure the underlying framework, and then, with the reticulum in full view, to compare its quantity, distribution, and arrangement in the two varieties of tumor, in order, finally, to come to a conclusion as to whether a study of this reticulum enables us to differentiate microscopically between lympho and round-cell sarcoma.*

I. Reticulum — Its Structure, Nature, and Distribution

It has been known for many years that in addition to the interlacing connective-tissue fibers of the various organs, there exists a separate network. Donders (1), Kölliker (2), and Billroth (3) have all described this network, which, owing to its peculiar reticulated appearance, was named "reticular tissue" by Kölliker.

The term "reticulum," then, was at first only a morphological one, used to characterize a type of connective tissue which differed from all other types, in that it was more distinctly reticulated. In tracing the development of ideas concerning reticulum, it will be seen that the morphological viewpoint has been gradually displaced by the chemical one. Billroth (3) was the first one to publish an extensive research concerning this tissue, which he studied in the spleen of various vertebrate types. He came to the conclusion that reticulum is made up entirely of anastomosing, branching cells, and he described the network formed by these cells as fine, pale, delicate, containing but few nuclei.

The views expounded by Billroth stood uncontested until 1859, when Henle (4) stated his conception of reticulum. Henle examined lymph-nodes, the lymph-follicles of the stomach and intestine, the tonsil, thymus, and spleen, and came to the conclusion that reticulum was *not* a cellular structure, but that it was made up entirely of fibers, which were of varying thickness, and which formed a meshwork that varied in density in different places. Nuclei never occur in the fibers, and what Billroth took to be nuclei were either artefacts or the places where fibers crossed each other. Henle further taught that reticular tissue should not be considered as a special variety of connective tissue, because, "in reality, there is no other kind of connective tissue than reticulated connective tissue; even the dense parallel fibers of tendons are in reality reticulated tissue, because the primitive bundles freely anastomose with each other."

Here are two diametrically different views concerning the structure of reticulum, both based on histological detail, and both vigorously contested almost up to the present time. Very careful work done by Bizzozero (5) and Ranvier (6) confirmed the view formerly ex-

pressed by Henle regarding the non-cellular nature of reticulum, and finally the work of Mall (7), Spalteholz, and Hoehle (8) established the fact once for all, that the network was made up entirely of fibers, and not of anastomosing, branching cells.

The first one to take issue with Henle regarding the nature of reticulum, and to study its chemical as well as its histological properties, was His (9). His subjected lymphoid tissue to the action of dilute alkalies and acetic acid, and allowed them to putrefy. He noted that whereas the parenchyma of the lymphoid tissue was but slightly resistant, and therefore easily destroyed, the reticulum, on the other hand, maintained its shape and form, except in so far as it became somewhat swollen and a little paler than usual.

The work that established the identity of reticulum as a specific type of connective tissue beyond all doubt was done by F. P. Mall (7). In his paper, "Reticulated Tissue, and its Relation to Connective-tissue Fibrils," Mall shows that by the action of KOH, pepsin, pancreatin, papain, bacterial putrefaction, and hot and cold acids, it is possible to differentiate between elastic tissue on the one hand and white fibrous and reticular tissue on the other. This differentiation is made with comparative ease, and with the necessity of but little refinement in technique. Regarding the final differentiation between white fibrous tissue and reticulum, Mall says: "The reactions of white fibrous and reticulated tissues are closely related in so many respects, that it is convenient for me to consider them together." That Mall did not mean to imply that differentiation between white fibrous and reticular tissue is impossible, may be seen from the following table that he arranged to show methods of differentiating the two kinds of tissue. (*Johns Hopkins Hospital Reports*, vol. i, p. 182.)

	Pancreatin	Papain	Boiling Acid	Boiling KOH	Pancreatin, Osmic Acid Boiled, then Pancreatin	Magenta Stain	Yields Reticulin	From a Solution Made by Boiling with Dilute HCl	
								Sugar of Lead	Alcohol
Elastic tissue	+	+	o	o	+	+	o	o	o
White fibrous tissue	o	o	+	+	+	o	o	o	Precip.
Reticulum..... ..	o	o	+	+	o or ×	o	+	Precip.	o

It will be seen from this table, that whereas reticulum is not soluble, or only slightly soluble in pancreatin, yields reticulin on boiling, is not precipitated by alcohol, and is precipitated by sugar of lead from an HCl solution, yet white fibrous tissue is dissolved completely by pancreatin, yields no reticulin, is precipitated by alcohol, and not by sugar of lead. Spalteholz (8) makes the positive statement, "für Collagenes und reticulirtes Gewebe, ist die trypsin Verdauung eine sichere chemische Reaction."

Such, then, is the histological structure and chemical nature of reticulum. That we are not yet clearly in the light concerning this tissue may be inferred from Spalteholz's (8) statement that we cannot yet differentiate reticulum and collagenic tissue, and from Mall's introductory sentence, "I propose to show that a tissue, *practically identical with reticulum*, is widely distributed through the body, and that it is neither white fibrous nor yellow elastic."

Concerning the distribution of reticulum in the body, Mall reports having found it in lymph-nodes, spleen, mucous membrane of the intestinal tract, kidneys, liver, lung, bone-marrow, pancreas, testicle, thymus, heart muscle, and thyroid. It is absent in bone, cartilage, and the whole of the nervous system.

The conclusions that Mall draws from his observations are:

"We have, therefore, three important groups of tissue to form the framework of the body.

"1. The fibrils of the white fibrous tissue are arranged in bundles or membranes of various strengths, which are resistant enough to withstand the unequal pressure or stretching which may be exerted upon the body.

"2. The elastic fibers branch and again unite, thus forming networks, which again expand under pressure, but, when the pressure is removed, return to their normal position without taxing the forces of the body.

"3. The reticulum fibrils form the finest network, in whose meshes lie cells, well protected when the volume of the organ is suddenly changed.

"It remains for physiology to show the part each group of fibrils plays, when they are side by side, in portions of the same organ."

THE OCCURRENCE OF RETICULUM IN NEW GROWTHS

As we have seen from Mall's work, reticulum is widely distributed throughout the body. Since all neoplasms must originate in one of the three primary embryonal layers of tissue, we should expect, *a priori*, that reticulum would be present fairly constantly in all new growths. Unfortunately, however, but very little work has been done to confirm this *a priori* assumption. It is true that numerous authors have described the occurrence of reticulum in new growths. Börst, in his *Geschwulstlehre*, describes the reticulum of the stroma of lympho and round-cell sarcoma, and describes the reticulum occurring in other tumors; Kaufmann likewise makes detailed mention of the reticulated network of sarcomata in his *Pathologische Anatomie;* Ackermann (10) gives an excellent description of the reticulum of spindle-cell sarcomata; and Ribbert (11), following up the work of De Bruyne (12), describes the reticulum occurring in rhabdomyomata. Most of this work was done, however, before the fact was established that reticulum is a specific type of connective tissue, and, as a result, the term "reticulum" is used only in the morphological sense. Deductions based on such observations lose a large part of their value by reason of the fact that what these authors have described as reticulum was in reality a mixture of white fibrous tissue, yellow elastic tissue, and reticulum. Moreover, only one attempt has ever been made, as far as I can make out, to differentiate tumors on a study of their bared reticulum.

The paper of White (13) stands alone as the only attempt to study the reticulum of new growths, along the lines laid down by Spalteholz and Mall, for differentiating reticulum from the other varieties of connective tissue. White studied the reticulum in seventeen new growths — ten carcinomata, three round-cell sarcomata, one spindle-cell sarcoma, one cystoma, and three fibromyomata. Although he quotes the work of Siegfried (14) as proving that the reticulin yielded by reticulum differentiates this tissue from white fibrous, he nevertheless holds the differentiation to be unsatisfactory, and prefers to consider white fibrous tissue and reticulum together. As a result of his studies, he found that reticulum was present in all the tumors examined, but that, as a rule, sarcomata show an exceedingly fine intercellular network of reticular structure, very similar to the reticulum present in normal glandular tissue, whereas carcinomata have no intercellular network. He suggests, also, that this fact might be used as a point of differentiation between carcinomata and sarcomata in doubtful cases.

II. Pancreatin Digestion — Technique and Results

In order to obtain a full view of the reticulum, one of three procedures is necessary to get rid of the cells that rest on and obscure the underlying framework. 1. A portion of the tumor may be cut by the frozen method and the sections well shaken in a test-tube of water, thus setting the cells free from their connections by a purely mechanical process. 2. The specimen may be imbedded in paraffin, sections cut, placed on glass slides, and then gently stroked with a camel's-hair brush, in order to brush the cells loose. 3. The specimen may be digested in a solution of pancreatin. Both 1 and 2 possess, in common, the disadvantage that they do not rid the section of *all* the cells; for neither the mechanical process of shaking nor of brushing serves to loosen those cells, which are rather firmly bound up in the reticulum. Moreover, the reticulum, as may be seen from the accompanying plates, is such a very delicate structure, that it is practically impossible to shake or brush the sections with any degree of force without breaking numerous fibers, thus creating undesirable artefacts. For these two reasons, then, the desire to do away with *all* the cells, and the object to preserve the reticulum absolutely intact, I selected the method of pancreatic digestion.

Pancreatin, an enzyme of pancreatic juice, possesses the power of digesting proteids in a weak alkaline solution of from 0.2 per cent to 0.4 per cent bicarbonate of soda. The process of digestion goes on best at a temperature of from 37° to 40° C., and is stopped by a temperature of 50° C. This 0.2 per cent to 0.4 per cent alkaline solution, at a temperature of 37° C., digests the proteid bodies, nuclein, mucin, and elastic tissue of the higher vertebrates, but does

Plate I. Normal tonsil. A single follicle. ×30.

not, according to Spalteholz, affect the collagenic tissue, reticulum, chitinous substance, cornified tissue, or fat. Treatment with various fluids, before using the pancreatin, greatly influences the digestion; for example, specimens fixed in various solutions, such as Müller's, Orth's, or Zenker's fluid, formalin or alcohol, all react differently to the digestive ferment. According to the various authors quoted by Spalteholz (8), there seems to be a rather wide range of opinion as to the effect of these different reagents. In my own work, I found, after experimentation, that digestion was most rapidly and thoroughly accomplished by fixing the specimens in seventy per cent alcohol before putting them in the pancreatin solution.

When pancreatic digestion was first used, the pancreatic solution was prepared by extracting the pancreas of an ox or a cow with an alcohol-ether mixture. Now, however, the commercial preparations of pancreatin are used. Here, again, care must be used in selection, preferably by testing the various preparations on a bit of fibrin. I obtained my best results with Grübler's pancreatin, which dissolves readily in the slightly alkalinized water, giving a clear solution. A murky, muddy solution or emulsion creates great inconvenience, in that it is difficult to find the piece of tissue which has been put into it for digestion. It is always best to add a few drops of thymol or chloroform to the pancreatin mixture in order to prevent the occurrence of putrefaction.

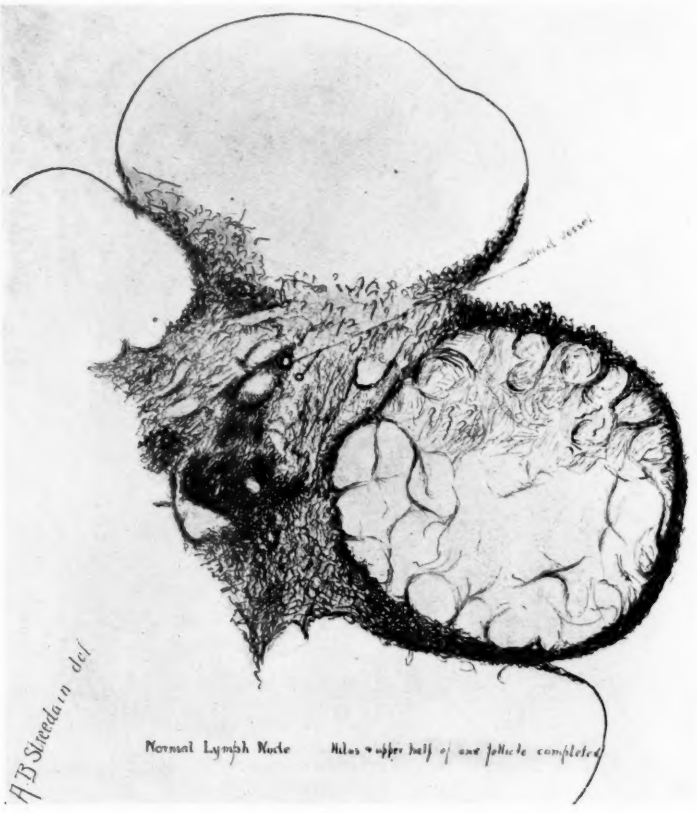

Plate II. ×40.

The various methods that have been devised for using artificial pancreatin digestion, in accordance with the type of tissue to be digested, are given in detail by Spalteholz in the *Encyclopädie der Mikroscopischen Technik;* but, for our purpose, only two of these methods need be considered, viz., the methods of Mall and of Spalteholz.

Mall's (7) method is as follows: Pancreatin is mixed with twice its weight of bicarbonate of soda and then the mixture dissolved in ten to twenty times its bulk of water, and kept in an incubator at 37° C. The tissue of which the reticulum is to be studied is obtained fresh, and frozen sections are cut from it. These sections are then placed in the pancreatin mixture above described, and allowed to stay there until thoroughly digested. After the process of digestion is completed, the sections are shaken in a test-tube of distilled water, in order to remove all granules, and then one of the digested sections is spread on a glass slide, where it is allowed to dry. It is then moistened with a drop of the following solution:

Picric acid	10 grams
Absolute alcohol	150 c.c.
Water	300 c.c.

The evaporation which takes place at this time is beneficial, rather than injurious. The excess of fuchsin is now poured off, and the whole slide placed in a large quantity of the picric acid solution for a very short time, and then transferred to absolute alcohol. The re-

Plate III. Round-cell sarcoma. Optic nerve. ×50.

ticulum is stained a beautifully brilliant red. The section is cleared in xylol and mounted in balsam.

Spalteholz's method (8) is as follows: Only well-fixed material is used, fixation being accomplished by using alcohols of increasing strengths, from fifty per cent up to one hundred per cent. Then the tissue is digested *en masse* (when *en masse* digestion is used, the tissue must be first freed of fat in a Soxhlet apparatus), washed in water, fixed again in the various alcohols, and imbedded in paraffin. Sections are then cut, mounted, and stained. Hoehle (8), a pupil of Spalteholz, modified the method somewhat, in that he first imbeds the tissue in paraffin, cuts it into sections six to ten micromillimeters thick, mounts the sections on glass slides, and digests them *in situ*. Spalteholz himself speaks very favorably of this modification of his method.

Mall's method is open to criticism on the following grounds: In frozen-section work, it is extremely difficult to avoid freezing too much or too little. If the block of tissue is too hard, delicate fibers break as the knife cuts through them; if, on the other hand, it is too soft, the knife drags its way through, tearing the fibers. Shaking the delicate digested sections in water, moreover, as Mall does, is apt to distort and tear the fibers. The process of mounting the

Plate IV. Round-cell sarcoma. Muscle. ×50.

flimsy sections is difficult; it is not easy to keep them on the slide during the process of staining, and the necessary manipulation is apt to create tears and distortions. These are difficulties that I encountered; yet it must be borne in mind that a method that is good in the hands of one experimenter may, for some unaccountable reason, fail in the hands of another.

Spalteholz's method of digesting a small piece of tissue and then imbedding and sectioning it gives excellent results, and, as far as I can see, is open to criticism only on the ground that the whole piece of tissue is digested, thus affording no means of comparing the digested sections with undigested ones taken from the immediately neighboring region. Hoehl's modification does away with this objection, for alternate sections may be mounted and stained in an undigested state. I found it, however, practically impossible to get the paraffin sections to adhere to the glass slide after they had been in the pancreatin solution for a few hours. The following method was thereupon devised and used without encountering any difficulty. From the tissue to be examined I cut a cube four millimeters each way. This cube was then bisected and cut into two parts, along the line *ab*. The portion *ac db* was imbedded in paraffin, and sections taken along the surface *ab* to be used for controls. The portion *ae bf* was digested *en masse*, imbedded, and cut also along the surface *ab*, the sections being mounted and stained with Mall's picric acid — acid fuchsin stain. Thus the undigested and digested sections (I used only the first few sections from each block, and I cut them about five micromillimeters thick) were nearly enough from the same portion of the tumor to serve as controls, each for the other. The only precaution to be observed in this method of "mass digestion"

Plate V. Round-cell sarcoma. Tonsil. ×65.

is, not to have the pieces of tissue too large. The size pieces used in this work were completely digested in three days.

III. Comparison of the Reticulum in Lympho and Small Round-cell Sarcomata

In attacking the problem of comparing the reticulum in lympho and small round-cell sarcomata, one is confronted at the outset with two embarrassing difficulties. In the first place, there is the difficulty of obtaining material. Even our largest hospitals hardly afford a sufficient number of these two tumors during the course of a year or so to serve for an extensive comparison. Unfortunately, most of the tumors that are excised, these days, are submitted to examination and then stored away in formalin. Formalinized tissue does not digest in pancreatin, and is therefore not suitable for the line of work outlined in this paper. It is important, moreover, to be absolutely sure of the microscopical diagnosis before attempting a comparison between these two types of tumors; and in order to be certain, one must have reasonable assurance that the microscopical diagnosis coincides with the clinical history of the case; in other words, one must be certain that his material comes from reliable sources.

Through the generosity of friends connected with a number of our American pathological laboratories and with some of those of Germany, I was able to collect about thirty-five specimens of lympho and small round-cell sarcomata, which had been preserved in alcohol immediately after their removal from the body. For these specimens I am greatly indebted to Dr. Eli Moschowitz of New York, Dr. J. H. Wright of Boston, Dr. O. Hensel of New York, Dr. C. Fisch of St. Louis, and Dr. Ludwig Pick of Berlin. By way of illustrating the difficulty of obtaining suitable material, it may be said that Professor von Recklinghausen of Strassburg had an assistant go through his pathological collection, only to find that all the desirable tumors had been fixed either in formalin, or in one of the chromic acid preparations. According to my own experience, and also according to Ewald (15), tissue fixed in the chrome salt solutions and exposed to the light for any length of time becomes absolutely resistant to the action of pancreatin.

The second difficulty to be met is the defectiveness of our knowledge concerning reticulum. Having digested a specimen of tissue, shall we

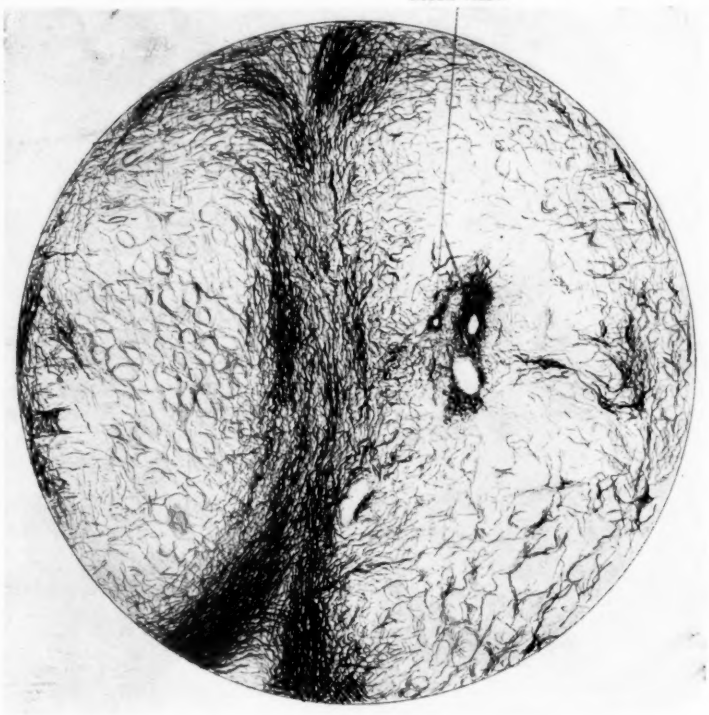

Plate VI. Round-cell sarcoma. Groin. (Lymph-node.) ×50.

consider the remaining network as pure reticulum or of reticulum plus white fibrous connective tissue? And if we do consider it as made up of these two elements, may we nevertheless draw conclusions of any value? Guided by the results of Mall, Spalteholz, Hoehle, and White, one is forced to accept the alternative of considering the network remaining after digestion as made up of reticulum plus white fibrous — or, according to Spalteholz, collagenic — tissue. This assumption, however, in no way affects the value of any conclusions that may be drawn, for, as has already been stated, the problem in hand is merely to compare the bared reticulated framework of lympho and small round-cell sarcomata.

Sixteen specimens of round-cell sarcoma and eighteen specimens of lymphosarcoma from various regions of the body were digested. Some of the lymphosarcomata were lymphomatous tumors taken from cases of leucæmia and pseudo-leucæmia. As a means of control, a normal human tonsil and lymph-node were also digested, in order to study the normal arrangement and distribution of reticulum in these two organs. Although the widest possible variation was found in the reticulum of the various tumors, it was nevertheless possible to select four specimens of both types, that embodied practically all the variations shown by the remaining twenty-six specimens. The ten plates which are appended show digested sections of a normal tonsil, a normal lymph-node, four lymphosarcomata, and four small round-cell sarcomata. These drawings were all made with aid of a camera lucida, in order to secure as accurate a reproduction of the specimens as possible.

The chemical nature of reticulum being irrelevant to the immediate problem in hand, it was left out of consideration, and the framework was considered only from the points of

Plate VII. Lymphosarcoma. Testicle. ×65.

view of its comparative quantity, configuration, and arrangement in the two types of tumor.

QUANTITY

Those authors who have studied the framework of sarcomata without the aid of the process of artificial digestion have agreed that the amount of connective tissue present varies not only in different tumors, but even in different parts of the same tumor. (Börst, Ackermann, Kaufmann.) Likewise, Mall and White, in their digestion experiments, reached the same conclusion. In my own work, I digested, alternately, a round-cell and a lympho sarcoma. The first tumor digested was a round-cell sarcoma of the muscles of the thigh, represented in Plate IV. The scantiness of the reticulum was a characteristic feature of this tumor. Here and there were spindle-shape accumulations of reticular network, between which there were comparatively clear fields, with only occasional single strands. This specimen was first compared with a lymphosarcoma of the testicle (Plate VII), and the tentative conclusion drawn, that in lymphosarcoma the reticulum is much denser than in round-cell sarcoma. Plate VII shows a reticulum that is dense everywhere, and in some places so extremely closely interwoven as almost to lose its reticular character. Further comparisons, however, showed that the element of quantity was such a variable one that it could not be used as a basis in differentiating between the two types. In a round-cell sarcoma of the optic nerve (Plate III) the reticulum is extremely delicate and scanty in places, whereas in others it is so dense as to appear as a solid, almost structureless tissue. This one specimen, in itself, not only teaches the fact that the quantity of reticulum is not dependent upon the variety of tumor, but it suggests a far more important conclusion; namely, that the reticulum may be of autoc-

Plate VIII. Lymphosarcoma. Mesentery. ×35.

thonous growth in the tumor itself. According to Mall (7), "the whole nervous system contains no reticulum." Yet here, in a tumor originating from a nerve, reticulum is present in abundance. There are but two possible explanations for this phenomenon; either the reticulum forms hand in hand with the growth of tumor parenchyma just as it does in granulation tissue, or else the small quantity of reticulum or white fibrous tissue of the perineurium and endoneurium undergoes a proliferation which supplies the tumor with a well-developed network. At all events, the fact that there is no reticulum in the whole nervous system does not carry with it the corollary that sarcomata originating from the nervous system also contain no reticulum.

In the two tumors already compared, the round-cell sarcoma showed a poorly developed reticular framework, whereas the lymphosarcoma was extremely rich in reticulum. In order to convince ourselves that the deduction from this comparison cannot be used as a fixed standard, we need only compare a lymphosarcoma of a lymph-node (Plate IX) with a round-cell sarcoma of the tonsil (Plate V). As a result of this comparison, we see that the order of things is reversed; the reticulum is extremely dense and abundant in the round-cell tumor and markedly scanty in the lymphosarcoma. The comparison is all the more valuable, considering that we are dealing with two different tumors affecting the same variety of tissue (lymphoid). A study of the round-cell sarcoma of an inguinal lymph-node (Plate VI) serves even more clearly to show that we cannot establish any fixed law regarding the quantity of reticulum; for in this specimen the reticulum in places is as dense as in the round-cell sarcoma of the tonsil (Plate V), and in others as scanty as in the lymphosarcoma of the lymph-node (Plate IX).

Plate IX. Lymphosarcoma. Lymph-node. ×50.

The explanation for the seeming irregularity in the distribution of the reticulum is easily appreciated after studying digested sections of a normal tonsil and lymph-node. Plate I represents a digested section of tonsil, and Plate II a section of a normal lymph-node. A glance at either one of these sections shows that, normally, reticulum is unevenly distributed. At the hilum of the lymph-node and at the periphery of the tonsillar follicle it is very abundant, whereas it is very scantily supplied to both the follicle of the lymph-node and to that portion of tonsillar follicle bordering on the crypt.

It is clear, therefore, that in considering the quantity of reticulum in either a lympho or round-cell sarcoma we are, of necessity, forced to take into consideration the quantity of reticulum normally present in the tissue or organ from which or in which the tumor grows.

CONFIGURATION

A careful study of the arrangement of the fibers shows a tendency on the part of the reticulum to arrange itself in four ways:

1. Whorl formation.
2. Irregular lattice-work.
3. Radiations from focal points.
4. Atypical configuration.

Whorl Formation. This is best illustrated in the lymphosarcoma of the cæcum (Plate X), where the arrangement is so marked as to lend character to the whole section. This arrangement was also seen by White (13) in the sarcomata that he worked with. A study of the control specimens which were undigested showed that the empty spaces of the whorls were formerly occupied by cell-nests, a fact also confirmed by White. At first sight it appeared that the open spaces formed by the whorls were blood-vessels; but the large number of these spaces usually present, and the

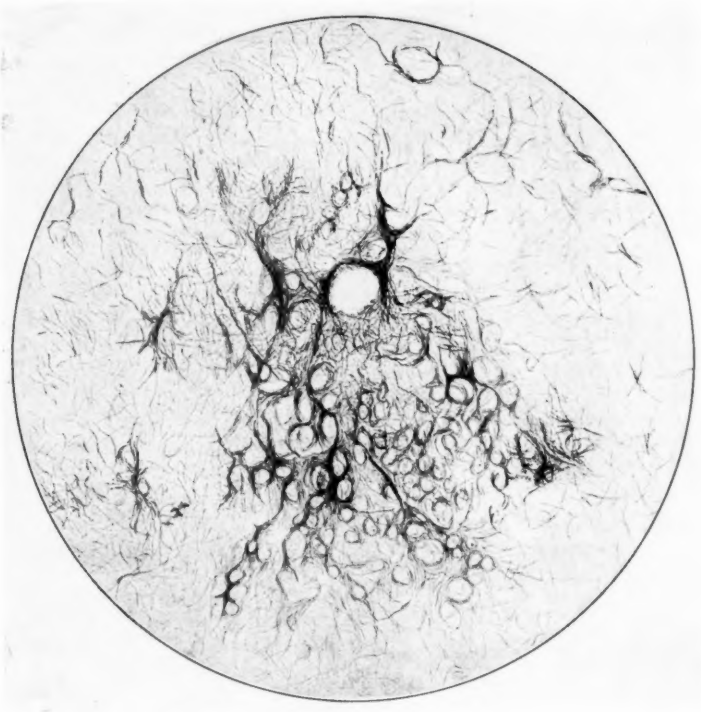

Plate X. Lymphosarcoma. Cæcum. ×75.

marked variations in their caliber, prompted an examination of the control specimens, with the result that no blood-vessels were found at sites similar to those occupied by the whorls. Moreover, a more careful examination showed that blood-vessels could be easily differentiated from the whorls, even in the digested specimens. The blood-vessels always appeared as clear-cut holes, usually surrounded by a dense reticular network, whereas the lumina of the whorls were almost regularly fringed with delicate reticulum, so that they presented an unshaved-beard appearance. The reason for this difference is apparent; for, whereas the blood-vessels in reality possess a lumen, which appears as such in the digested sections, the interior of the whorls is made up of a fine intercellular network. The fibers forming this network, owing to their tortuosities, are cut off short, and thus seem to fringe the whorl. As may be seen in Plate X, a large number of whorls are marked by fibers of the intercellular network which have not been cut off short, and which consequently course across the entire lumen of the whorl. The characteristic appearance of the blood-vessels may be well seen in Plates II, VI, and VIII.

Unfortunately, the presence of whorl formation does not aid us in differentiating the two types of tumor; for its occurrence is not limited to one type. In Plates IV, V, VIII, and IX, it is absent, and of these four tumors, two are round-cell and two are lymphosarcomata. It is well marked in Plates III, VI, VII, and X, and here, again, two are lympho and two are round-cell sarcomata.

Irregular Lattice-work Formation. This is the ordinary appearance of reticulum, and is present, without distinction, in some portion, at least, of every section. Its presence, therefore, is also of no aid in differentiating the two types.

Radiations from Focal Points. This radiation from focal or nodal points occurs wherever numerous fibers cross each other, and may be well seen in Plates III and X. It represents one of the earliest noted facts concerning reticulum, and was the subject of much discussion in the papers of Billroth and Henle. It may be assumed to occur wherever reticulum is found, be it in normal organs or in neoplasms. There is a peculiar form of this nodal tendency present in some of my specimens, however, which has never before attracted attention. It is probably best referred to as a sort of polarity of the fibers, which seem to sweep around and arrange themselves, much as do the lines of force about the end of a magnet. This tendency is particularly well seen in Plates V and VIII. Here, again, one of the specimens is a lympho and one a round-cell sarcoma, so that the polarity affords us no differential aid. In some of the digested specimens the tendency of the fibers to sweep about and droop gave a picture very similar to that of a weeping-willow tree.

Under the head, "Atypical Configuration," are grouped those specimens in which the fibers conform to no definite arrangement. The lymphosarcoma shown in Plate IX illustrates this atypical arrangement of the fibers. Here and there a whorl formation may be seen, here and there attempts at radiation, but, on the whole, the fibers are distributed in a haphazard fashion, dense fibers lying side by side with finer ones. The configuration is, as its name signifies, so atypical that it affords no definite basis of differentiating the lympho from the round-cell type of tumor.

ARRANGEMENT

The results of the comparative study of the quantity and configuration of the reticulum in the two types of tumor were negative, in that they afforded no definite basis of differentiation. It was thought that a special study of the arrangement of the fibers might possibly lead to a useful clue. The object of this study of the arrangement was to determine whether the normal arrangement of the fibers, as seen in the various organs, is preserved or lost in tumors originating in these organs. But here, too, the results were negative. Plate II shows the normal arrangement of the reticulum in a lymph-node, with the characteristic densely interwoven fibers at the hilum and periphery of the follicle, and the loosely interwoven network at the center of the follicle. Plates V, VI, and IX all represent tumors arising from lymphoid tissue, and yet the arrangement in none of them resembles the normal lymph-node arrangement. As has already been stated, the nervous system contains no reticulum, yet the digested specimen of the optic-nerve sarcoma shows a dense reticulum. In the intestine, the reticulum occurs chiefly as a network of fibers originating in the muscularis mucosæ, and winding themselves about the crypts up into the villi (Mall) so as to give a characteristic picture. The digested specimen of a lymphosarcoma of the cæcum (Plate X) shows a multiplicity of whorls, rather than anything characteristic of the reticulum of normal intestine. A study of the tumor of the testicle, however, yields different results. In a normal testicle, the reticulum is arranged about the convoluted tubules and efferent channels. In the specimen of lymphosarcoma of the testicle represented in Plate VII, the tubules were so choked with lymphoid cells that it was impossible to distinguish their outlines clearly; yet in the digested specimen it will be seen that the tubules stand out as clear spaces, fringed on the inside with shortly cropped-off reticular fibers.

On purely *a priori* grounds, two assumptions were possible:

1. That the reticulum in tumors would *not* conform with the normal arrangement of reticulum, because the characteristic tendency of all neoplasms is to manifest a wild, unrestrained, and limitless growth, participated in both by the parenchyma and stroma of the new growth.

2. That, since some tumors manifest a distinctly mimetic tendency (such as the adenomata and adenocarcinomata), it is possible that the reticulum in sarcomata may assume the arrangement of the reticulum of the organ from which the tumor springs. A study of the digested specimens, however, shows that the normal reticular arrangement may be preserved or lost in different tumors. It is unnecessary to state that in comparisons of this sort, only specimens taken from the site of origin of the tumor are of any value.

CONCLUSIONS

I. The quantity of reticulum present in small round-cell and lympho sarcomata varies within such broad limits and with such irregularity that it is impossible to associate definitely either type of tumor with the amount of reticulum it contains.

II. The reticulum in both lympho and small round-cell sarcomata assumes a fairly definite configuration, which may be classified under the heads, Whorl Formation; Irregular Lattice-work; Radiations from Focal Points, and Atypical Arrangement; but none of these configurations is so specifically limited to either type of tumor as to be of any differential diagnostic aid.

III. The arrangement of the reticulum usually conforms to one of the above four configurations, rather than to the normal arrangement of the reticulum of the organ involved. It is possible, however, for the normal reticular configuration to be preserved.

IV. Pancreatin digestion of small round-cell and lympho sarcomata affords a clear study of the reticular framework of these tumors, but from this study we are not able to deduce facts by means of which we may differentiate the two tumors histologically.

V. A study of the reticulum in lympho and small round-cell sarcomata of lymph-nodes revealing no histological differences, it is warranted and expedient to group these two types of lymph-node tumors under the one head of malignant lymphomata.

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AKANTHOSIS NIGRICANS

Further Remarks on Akanthosis Nigricans, a Pathognomonic Symptom of Intra-abdominal Carcinoma, and Kausch's Œsophageal Narcosis-tube, Designed for the Prevention of Fæcal Drowning

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OWING to an unfortunate incident, the illustrations representing Kausch's œsophageal narcosis-tube, as well as that showing the skin lesion called akanthosis nigricans, so frequently seen in intra-abdominal carcinoma, could not be embodied in my paper on "Cancer of the Colon," read at the Cleveland meeting of the American Surgical Association, May, 1906, and appeared in the August SURGERY, GYNECOLOGY AND OBSTETRICS.

Considering these illustrations of some value in connection with the subject, I am enabled, through the courtesy of the editor, now to present them with the following few remarks:

1. *Akanthosis Nigricans (Pollitzer)*

The accompanying colored drawing represents a faithful picture of the abdomen of a woman, 56 years of age, with cancer of the pancreas and secondary infiltrated retroperitoneal lymphatic glands, observed at the German Hospital this summer. The many capillary angiomata, smaller and larger brownish spots not unlike freckles, and flat, wart-like skin infiltrations had appeared in the early part of 1906. During the last 18 months she frequently complained of pain across the abdomen. A large, nodular, slightly tender, hard mass was palpable just below the border of the left costal arch. It seemed slightly movable and easily detectable on bimanual palpation. The sensation of intestinal stiffening had never been noticed by the patient. When ballooning the colon with air, the latter organ appeared to lie in front of the mass, which made the otherwise most plausible diagnosis of cancer of the splenic flexure and descending colon somewhat doubtful. Catheterization of the ureters showed the left kidney entirely normal, with unimpaired working power. Although the patient did not present any cachexia, the diagnosis of intra-abdominal cancer was made. Incision showed carcinoma of the pancreas, with involvement of the lymph-glands.

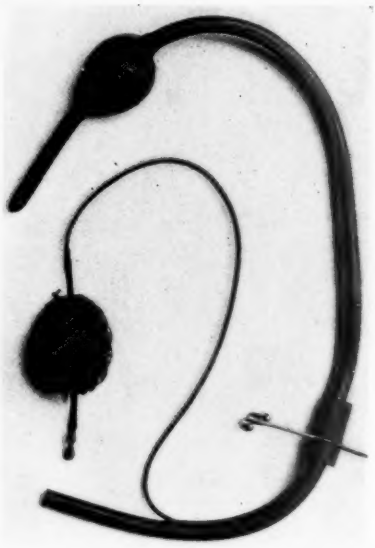


Fig. 2. Kausch's original œsophageal narcosis-tube.

The condition shown in the illustration (Fig. 1) is a typical and pathognomonic one, representing a mild case of akanthosis nigricans. The pronounced type, as described by Pollitzer, has not, so far, come under my observation.

During the last few weeks two other patients have come under my care, presenting the picture of a mild type of the skin affection under discussion, which, it seems to me, is certainly more frequently met with than the pronounced type.

One was a man of 63, afflicted with cancer of the lesser curvature of the stomach, constricting the cardia, with secondary involvement of the liver. The œsophageal stricture necessitated gastrostomy.

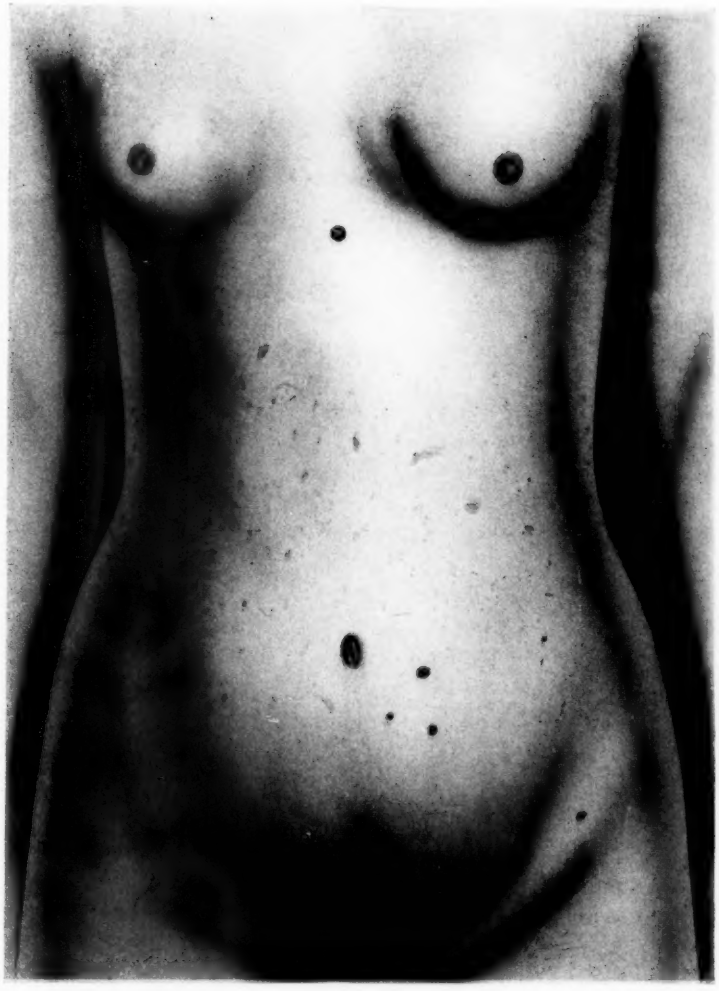


Fig. 1. Showing a mild condition of acanthosis nigricans. (Case 4.) Cancer of pancreas. Secondary infiltrated peritoneal glands.

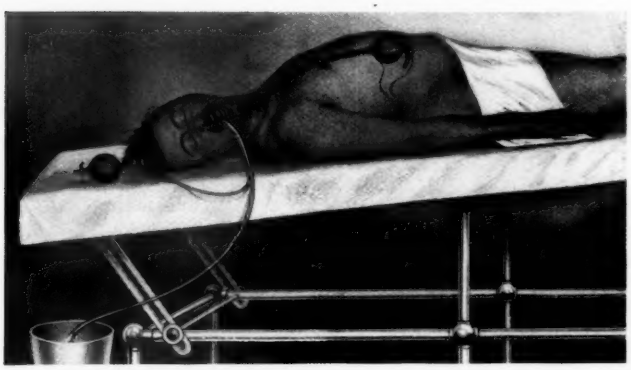
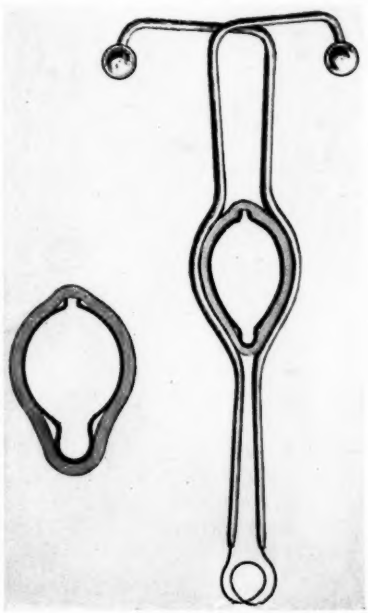


Fig. 3. The œsophageal narcosis-tube, slightly modified by author, in place during operation for intestinal obstruction. The lower bulb, properly distended, hugs the cardia. It must not be pulled into the œsophagus. The proper position of the tube has to be controlled by an assistant during the entire course of the operation.

The other, a lady 65 years of age, showed an infiltrating carcinoma, which seemed to start from the anterior wall of the hepatic flexure of the colon, and had spread to the gall-bladder and pylorus, surrounding the latter. Hæmoglobin, 43 per cent. Gastro-enterostomy and entero-enterostomy with the elastic ligature.

2. *Kausch's Œsophageal Narcosis-tube for the Prevention of Fæcal Drowning in Operations for Intestinal Obstruction*

In 1903, W. Kausch, then of Breslau, first presented this instrument. It is constructed on the principles of Trendelenburg's tampon canula for the trachea. As shown in the illustration (Fig. 2), an ordinary soft-rubber stomach-tube is made to pass through a rubber balloon. The latter, being 7 cm. in diameter when fully inflated, is securely fastened around the tube at both points of passage, the lower being two inches above the eye at the end of the tube. Emerging together with the stomach-tube from the upper end of this balloon, and passing along the same, is a thin rubber tube, which, a few inches from the outer end of the stomach-tube, separates from the same to enter another bulb of exactly the same size as the lower one, thus forming the connection between the two balloons at either end of the apparatus. From the opposite side of the outer balloon a small piece of thin tubing is made to project, for the purpose of inflation, which may be done by means of a hand-syringe. Before beginning the narcosis, this apparatus, with the lower bulb pressed empty and well anointed, is introduced into the stomach. Then the bulbs are distended and the small projecting tube is plugged or



a *b*

Fig. 4. Metal capsule and spring. The former (*a*), fixating the tube, prevents compression of the apparatus by the patient's teeth; the latter (*b*) secures the proper position of the distended bulb at the cardia, as shown in Fig. 3.

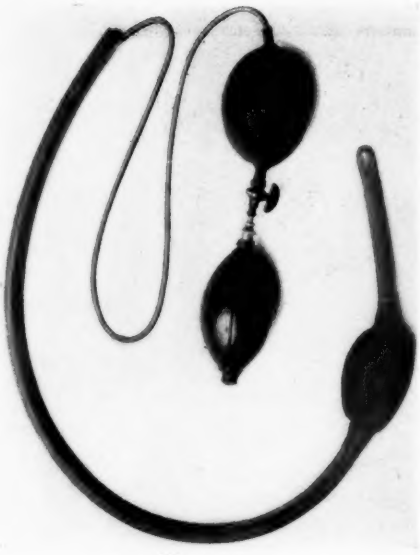


Fig. 5*a*.

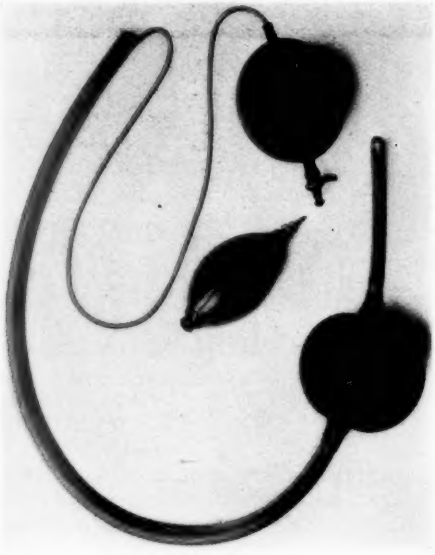


Fig. 5*b*.

Fig. 5*a*. The œsophageal narcosis-tube slightly modified, with bulbs collapsed; metal capsule, preventing compression by patient's teeth, slipped off. Fig. 5*b*. The œsophageal narcosis-tube with bulbs inflated; the feeder attached.

clamped. The narcosis-tube is pulled upward until it meets resistance, which means that the lower bulb presses against the cardial opening, thus rendering it impossible for any stomach contents to pass through the cardia; every drop has to find exit through the tube. (Fig. 3.)

To prevent compression of the apparatus by the patient's teeth, a metal capsule, with a smaller diameter than that of the tube, and covered with rubber, held in place by two irregularly shaped pieces of metal, which are connected by means of a spring, is slipped over the same, the smaller tube lying in a groove provided for it. (Fig. 4.) As soon as it is noticed, by pulling the tube upward, that the inflated inner balloon hugs the cardia well, the outside metal arrangement is pressed against the jaw of the patient (Fig. 3), thus insuring the continued proper position of the instrument. Anæsthesia may now be started. Care must be exercised, of course, to keep the balloons well inflated; but, as the outer balloon is of exactly the same size as the one placed internally, the former furnishes an exact index to the condition of the latter. It is necessary to have an assistant control the proper inflation of the apparatus during the entire operation. By continuously and gently pulling the tube upward in the same direction during the operation, he will guarantee the proper position of the tube.

If insufficiently distended and too much traction be employed, it is plain that the lower bulb might be pulled into the œsophagus, thus rendering doubtful the intended prevention of the passage of stomach contents, besides causing considerable distention and peristalsis of the œsophagus.

While the tube is *in situ*, the saliva cannot be swallowed. The patient's head should therefore be turned sideways, and the pharynx wiped out from time to time. A slight Trendelenburg posture facilitates the outflow of the saliva.

The instrument remains undisturbed until the patient has fully come to after anæsthesia. If it is properly placed, all the fluid of the intestinal tract, which continuously regurgitates into the stomach during operation, is carried through the tube into a receptacle placed underneath the operating-table. (Fig. 3.) Hence, the evacuation of intestinal contents by means

of multiple transverse incisions of the loops, with subsequent suture, which usually becomes imperative in order to accomplish reduction of the greatly distended gut after eventration, is rendered unnecessary, and consequently the danger of infection is materially reduced.

Furthermore, if the patient seems able to stand immediate radical work, the use of the narcosis-tube may also enable us to forego the necessity of primary enterostomy or the establishment of an artificial anus, and thus possibly prevent death from inanition in cases in which obstruction involves the upper part of the jejunum.

Figs. 5*a* and 5*b* show a slight modification of the original apparatus, which I asked Messrs. George Tiemann & Co., 107 Park Row, New York City, to make. The balloons are made of somewhat more durable rubber, better fitted for our climate, and instead of using a syringe to inflate the balloons and then clamping or plugging the outer end of the small tube, I have installed for this purpose a small detachable balloon as used in Paquelin's thermocautery, together with a small faucet for closure.

Kausch's œsophageal narcosis-tube, to my mind, represents a most important addition to our instrumentarium in operations for intestinal obstruction, so frequently accompanied by fæcal vomiting. It is the only apparatus at our disposal for the purpose of avoiding the much-dreaded fæcal drowning. Kausch has used the tube in three cases with perfect success. I have no doubt that it will find a wide field of usefulness.

A STUDY OF VAGINAL CÆSAREAN SECTION[1]

Considered with Especial Reference to Subsequent Pregnancy and Labor

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THERE is perhaps no other subject in the domain of obstetrics that has given rise to more spirited controversy than that of the status of vaginal Cæsarean section. Dührssen (1), who broadened its indications and developed its technique, is very optimistic as to its applicability. Pozzi regards it as the last great surgical feat of the nineteenth century, and says that no more valuable operation, since the introduction of forceps, has been introduced into midwifery. De Lee (2) believes it constitutes the most important addition to obstetrical technique contributed in the past thirty years. The literature in support of the operation is growing steadily; a number of congresses have regarded it favorably, and the one at Kiel in 1905 recognized its superiority over all other methods of vaginal delivery in closed or degenerated cervix.

Like many other innovations, however, the operation has been vehemently opposed as being an irrational, hazardous procedure, and even stigmatized as the prostitution of surgery. Bossi (3), the champion of instrumental dilatation of the cervix, deprecates it, and says it should be abandoned. Ahlfeld (4), Hofmeier (5), and others, contend that it is unnecessary, claiming that equally good results can be obtained by more conservative measures. Palmer Findley (6) says its only justification is possibly in rigid cervix; otherwise, if a cutting operation is elected, the classical method, on account of precision, asepsis, and expediency, should be given the preference. As a matter of fact, the truth of the question probably lies between these extreme views. To appreciate the sphere of each of the various methods of artificial cervical dilatation, for each has its specific indication, one must have in mind the anatomical changes and the physiological action of the uterus during pregnancy and labor. In the primipara, the cervical canal is closed throughout pregnancy; while in the multipara the internal os may be closed, the

[1] Read before the Southern Surgical and Gynecological Asscciation, December 11–13, 1906.

external is patulous. In the later months of gestation in the primipara, owing to the increasing intrauterine tension on account of the intermittent contractions and resistance of the cervical barrier, the passive lower segment is formed; and at or just prior to labor the forces are also exerted upon the internal os, which gradually unfolds until complete dilatation of the cervix is effected. In the multipara, however, because of the relaxed condition of the uterus, the intrauterine pressure is not so great, except in hydramnios and twin pregnancy, so that there is little effacement of the internal os during labor, and in dilating, the cervical canal assumes rather a tubular form, unlike that of the primipara, which is conical.

Since vaginal Cæsarean section is not subject to such restrictions, the operation is indicated, according to Dürhrssen, when the cervix is intact or imperfectly dilated, and not yielding to gentler measures, and the life of the mother or child is in danger, as from pathological conditions of the lower segment, stenosis, acute toxæmia, eclampsia, antepartal hemorrhages, heart and lung complications.

Dührssen (7) has collected 378 cases of vaginal Cæsarean section, with 48 deaths, or 12.7 per cent. He attributes this high mortality, not to the operation, but to the serious conditions under which the operations were undertaken. In 53 of the cases, the uterus was extirpated after the vaginal Cæsarean section. Only 5 died (9.4 per cent).

In this connection, it may be truthfully stated that general statistics are not always infallible as to the merit of a measure in any particular case, depending as they do upon environment, judgment, and skill of many operators. Rather should we judge the measure by its possibilities from the results obtained by the individual operator. For instance, in 33 cases of vaginal Cæsarean section for eclampsia, Veit had only one death, and Bumm had only one death in 40 consecutive cases.

The principal objections advanced against the operation are: 1. Injuries to the bladder, and danger of extension of the incision and opening into the peritoneal cavity; 2. Liability to hemorrhage and sepsis; 3. Rupture of the uterus in a subsequent labor. The first should be obviated by careful technique. If the child is premature, an anterior incision will provide sufficient opening for its passage. At term, however, it may be necessary to incise the posterior wall also, although Liepmann delivered a child weighing 3,950 grams, in which only anterior hysterotomy was performed. Further, in more than sixty cases operated on by Bumm and Liepmann, by the same method, none were torn beyond the incision. The circumference of the child's head at term is 30–34 cm., so that the opening of the cervix must be from 8–10 cm. in diameter; sufficient to permit of the passage of a large-sized fist. In a narrow vagina, deep vaginoperineal incisions may be necessary to facilitate rapid and safe delivery. The danger of infection is no greater than that in the use of the tampon, metreurysis, forceps, version, manual or instrumental dilatation. Hemorrhage has not infrequently occurred, but it was probably due to inertia, and no fault of the operation. In a study of 140 cases by Stamm (8), there was no alarming hemorrhage, and only two cases of infection. In the 248 cases reported by Dührssen, there is not a fatal result considered as due to hemorrhage. Dührssen's contention, however, that asepsis can be brought about in just as exact a manner by vaginal hysterotomy as by the classical method is not supported by the light of bacteriological investigation, which has demonstrated that while the uterus and vagina may be free from pathogenic bacteria, the vulva is rarely ever so, and it is almost impossible to render it sterile. Neither is his claim, that, with the proper technique, the child is not any more endangered in vaginal delivery than in delivery by the abdominal route, in accord with the experience of most authorities.

The claim of liability to rupture of the uterus in subsequent labor is not supported by the cases thus far recorded. In searching the literature, and with the assistance of Professor Dührssen, I have been able to collect eleven cases of pregnancy and labor subsequent to vaginal Cæsarean section, to which I add one of my own. These are briefly stated as follows:

DÜHRSSEN. *Handbuch der Geburtshülfe*, von Winckel, 1906, bd. iii, th. 1. A case is reported in which, three years after an operation for vaginal fixation, the patient became pregnant, and at term the child presented trans-

versely. Complete cervical dilatation was accomplished in six hours by the Champetier de Ribes bag, and delivery effected by version and extraction.

WENNERSTRÖM. *Centralblatt für Gynäkologie*, 1903, No. 10. Frau K.; multipara; aged 28; incarcerated, retroverted, pregnant uterus; cessation of menses five months. The usual methods of correcting displacement proving ineffectual, and menacing symptoms manifesting themselves, posterior vaginal hysterotomy was performed, and a macerated fœtus 15 cm. in length was removed (about three months' gestation). Two years later, patient had a normal birth.

STAMM. *New York State Medical Journal*, 1906, vol. vi. Vaginal Cæsarean section for eclampsia in September, 1903. In July, 1904, patient gave birth to a 7½ months' child, before the arrival of a physician, and without any untoward result.

WESTPHAL. *Centralblatt für Gynäkologie*, 1906, No. 29. Frau X; iii-para; aged 23; eclampsia. Unsuccessful application of conservative methods; vaginal Cæsarean section performed. Post-partum hemorrhage controlled by tampon. On account of threatening collapse, exact suturing of incision was not possible; but the result, however, was excellent. Twenty-four hours afterwards, the convulsions had ceased, mind cleared, and on the following day, albumin had disappeared from the urine. Examination three weeks later revealed a small granulating surface of the anterior fornix and cervix. Four months after operation, again pregnant, 2½ months; œdema of face and limbs; albumin two per cent. Aborted at three months. Some weeks later, genitalia and kidneys normal. In May, 1905, became pregnant for the third time. No disturbance of kidneys; gestation progressed normally to an easy birth, without assistance, February 21, 1906.

RÜHL. *Münchener med. Wochenschrift*, 1906, bd. liii, h. 507. Frau X; November 19, 1903; primipara; aged 25. Anterior vaginal Cæsarean section for rigid os. During extraction, a transverse tear of the lower segment extending from incision into the parametrium occurred. Recovered. Saw patient again at the eighth month of pregnancy. *Examination:* Vertex presentation, first position; extending into left parametrium, a dense cicatrix was felt. He feared the delivery would be attended with great danger; but, much to his surprise, the birth was easy, of five hours' duration, and the child was living. The placenta was expelled spontaneously. Puerperium normal.

RÜHL. Ibid. Frau X, aged 29. At term, July 12, 1903; R. O. A.; vaginitis; rigid os; pains severe, but ineffectual; waters expelled after three days; unfavorable symptoms developing; temperature 39.3° C.; pulse 125. Vaginal Cæsarean section was elected, on account of expediency. Child living; puerperium morbid, and recovery slow. Patient returned two years later, and was three months pregnant. *Examination:* Uterus was drawn to left side and fixed. The organ was freed by opening parametrium and cutting through adhesions. Abortion was expected, but pregnancy continued to term, and labor was easy and quick; child living; puerperium normal.

RÜHL. Ibid. Frau X; primipara; December 5, 1903; eight months' gestation; eclampsia; œdema of feet; albumin marked. Anterior vaginal hysterotomy was performed. One convulsion after operation; puerperium normal. The next pregnancy was normal, terminating with an easy labor of six hours' duration. Adherent placenta, removed manually three hours after delivery. The puerperium was normal.

RÜHL. Ibid. Frau X; multipara; aged 31. Labor began January 15, 1904; waters expelled 24 hours later; in spite of all-day pains, cervix failed to dilate beyond diameter of finger. Colpeurynter introduced, with no increase in dilatation, after eight hours' continuous traction. The bag was removed and the case allowed to proceed without interference until the 21st, when the temperature rose to 38.9° C.; pulse 118. Anterior vaginal Cæsarean section was performed; child alive; puerperium normal. At next labor, early expulsion of waters; four days, strong pains, without dilatation. Hot douches, atropia, morphine, etc., were given, and at the end of six days, only three fingers could be passed through the cervix. The Bossi dilator was introduced, and during 35 minutes was screwed up to 8 cm., when a hemorrhage suddenly occurred, necessitating removal of instrument and immediate version. Extraction progressed slowly as far as head, when multiple cervical incisions were necessary to terminate the delivery. Placenta expressed, and post-partum hemorrhage controlled by gauze packing. There was an injury to the bladder, which closed spontaneously in three weeks.

VON BARDELEBEN. (Reported from Liepmann.) *Centralblatt für Gynäkologie*, 1906, No. 46. Frau X; multipara; aged 37; January 23, 1903; vaginal Cæsarean section performed for placenta prævia; six months' gestation. On examination, sixteen months later, was found to be four months pregnant. The subsequent labor was easy.

VON BARDELEBEN. Ibid. Frau M.; primipara; aged 24. On August 22, 1902, vaginal Cæsarean section was performed for eclampsia. Child living. Patient two years later had a normal pregnancy and a natural labor.

JERIE. *Handbuch der Geburtshülfe*, von Winckel, bd. iii, th. 1, 1906. Frau X; primipara; aged 23; eight months' gestation; eclampsia; 16 convulsions; 5 per cent albumin. Vaginal Cæsarean section; child dead; mother recovered. Two years later, normal birth.

At the last meeting of this Association (9), I presented by a title a report of two cases of vaginal Cæsarean section, performed for eclampsia, both successful, and I now report a subsequent pregnancy and labor occurring in one of them.

Mrs. F.; white; aged 27; i-para; 6½ months' gestation. Admitted to Columbian Hospital December 2, 1904, suffering with eclampsia. After five hours' treatment with morphia, etc., proving ineffectual, patient having had eight convulsions, coma deepening and cervix undilated, anterior vaginal hysterotomy was performed, and delivery terminated with forceps, and placenta expressed. Child lived thirty hours. Puer-

perium normal. Saw the patient again, on account of threatening toxæmia, in consultation with the attending physician, Dr. Ball, in November, 1905, soon after the beginning of her second pregnancy. Careful supervision of pregnancy was exercised, the untoward manifestations disappeared, and gestation continued to 8½ months. The preliminary examination revealed a justomajor pelvis; external conjugate 22 cm., spines 27.5 cm., crests 30 cm.; child small, vertex presenting, first position. Labor began July 18, 8 P.M.; waters ruptured about midnight. The pains of the expulsive stage were controlled by chloroform, to prevent precipitate labor and possible rupture of the hysterotomy cicatrix. The labor was easy and of five hours' duration. Child living; weight 6¾ pounds. Puerperium normal. Examination November 11, 1906: Uterus in good position and freely movable; fornix normal; general health excellent.

The number of cases is perhaps too small from which to form any definite conclusion as to the effect of the operation upon the uterus in subsequent labors. Yet, from the outcome of those above related, it would appear promising. The writer believes the danger of rupture is averted by the passive state of the lower segment during labor favoring its integrity, and further, the through-and-through suturing of the incision at the time of operation secures a firm union.

Everke (10) attributes the liability to rupture of the uterus following the Sänger method to the decidua not being included in the grasp of the suture, and thinks that it is probable that in the contraction and retraction of the uterus the apposed sides of the incision become separated, forming a furrow of fibrous tissue covered by the peritoneum and mucous membrane. The writer has been able to verify this apparent change, in a labor following the classical section.

It cannot be denied that complications and sequelæ attend vaginal Cæsarean section; but that is also true of all the methods of accouchement forcé. Each has its value, and should be employed only after a thorough examination has determined its suitability. An exact knowledge of the condition of the cervix before acting is the essential prerequisite to success. The bougie, tampon, and metreurysis may be used when the cervix is undilated, provided time is not a necessary factor. Manual and instrumental methods should be applied only when the internal os is effaced or effacing. If applied while the cervix is intact, they act by divulsion, rather than by dilatation, with its consequent dangers of hemorrhage, laceration, and infection. It is in the latter class that vaginal Cæsarean section is particularly indicated, and, further, has the advantage that in case of urgency the uterus can be emptied at any period during pregnancy or labor in a given time. It is therefore a meritorious operation, and a valuable addition to our obstetrical resources.

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DEPARTMENT OF TECHNIQUE

AN AID TO INTESTINAL SUTURING[1]

(Double Suspension Loop)

By A. F. JONAS, M.D., Omaha, Nebraska

In presenting this brief communication there is no intention to bring forward a new suture or new material or its preparation. We have at our command a surfeit of suture methods; more than enough from which it is possible to select a proper and practical one in all cases where it is desired to cause a union of peritoneal surfaces for the purpose of re-establishing the intestinal lumen. No one will deny that the fundamental principle in intestinal surgery lies in seroserous approximation. In order to achieve this, the approximated surfaces must be practically in a normal condition. Any pathological change in the opposed surfaces weakens the intestinal coats, so that any suture, however well applied, may tear out and defeat primary union. Consequently we must have: 1. A sound, healthy intestinal wall; 2. The ends to be approximated must be made accessible; 3. We must have a strong sterile suture material, which is found in silk or linen, or the Pagenstecher thread; 4. We possess methods of suture, in the form of Lembert, Czerny-Lembert, Halstead, Cushing, and Connell procedures, that fulfill every requirement; 5. We need simpler aids to facilitate the application of sutures. The intestinal ends must be so held that suturing can be done with ease and rapidity, by some simple means that is always at hand.

Every form of apparatus or instrument has the objection that it is not always at hand, and does not suit all cases. In our work we have used such contrivances as bone and potato bobbins, gelatin capsules, catgut rings, Harrington's segmented rings, each one of which has advantages in special cases; but they all possess the objections that they are not always at hand. They require time for their manufacture or preparation. It would seem, therefore, if we could make use of a simple method that is always at hand, requires no special preparation other than the material used in the essential part of the operation — namely, a simple sterile thread — we should have an aid that is always at hand. We have found that suspension loops can be made with a thread that is practical, simple, easily applied, and superior to any contrivance with which the writer is familiar. The author disclaims any originality for the method. Because of its simplicity, no doubt many of you have made use of it in your intestinal work.

The method is carried out as follows: After the ends of the intestine to be sutured have been smoothly trimmed with scissors, the circumference is divided into three divisions, one of the points of division being at the mesenterial attachment. The intestinal ends are first brought together by a suture at the mesenterial attachments, carefully including the mesenterium in the suture; then,

at one of the three divisions in the intestine, a round, straight, or curved needle armed with a suture is passed in and out, including serosa and muscularis in the bite of the needle, leaving a free margin of one fourth of an inch for inversion, and then pass the needle with the same thread over to

[1] Read before the Western Surgical and Gynecological Association, at Salt Lake City, Utah, August 31–September 1–2, 1906.

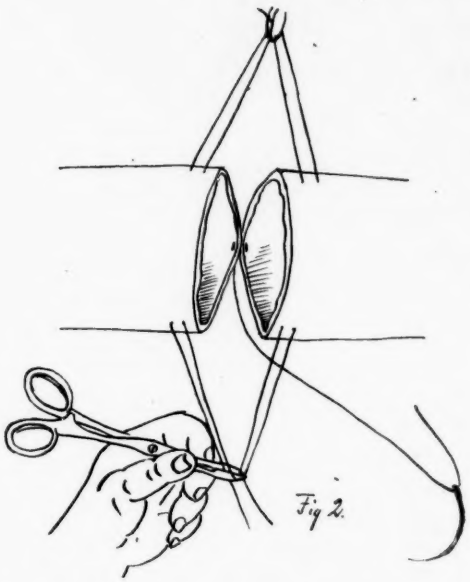


Fig 2.

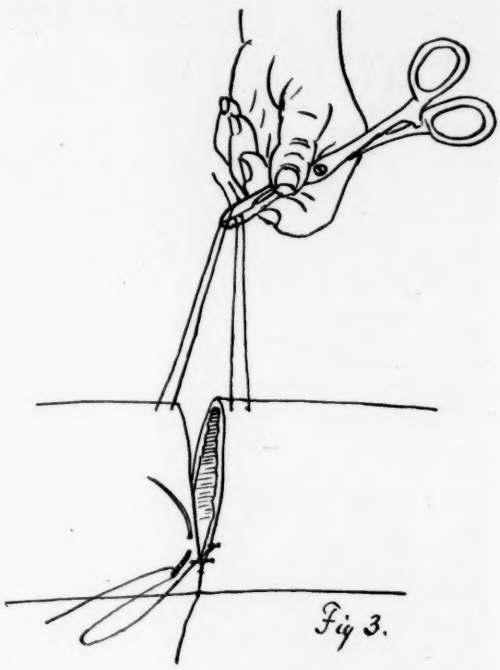


Fig 3.

the other intestinal end, piercing serosa and muscularis in the same manner as before; then remove the needle; now grasp that part of the thread which extends between the free intestinal ends and pull on it, forming a loop, as indicated by the lower thread in Fig. 1. By grasping the loop thus formed and catching the free ends of the thread, a double loop is formed, as indicated in Fig. 2. A second thread is passed in the same manner as the first one, forming a second double loop, as shown in Fig. 2. The threads are now grasped with hæmostats (Fig. 2), and when pulled taut, the intestinal ends are drawn together closely with a seroserous inversion. Now the suturing can be quickly and accurately done by whichever suture method may be deemed advisable. The writer has found the continuous Connell suture to serve him best. It has been applied in several cases, as shown in Fig. 3. While one of the double suspension loops was supported by an assistant a continuous through-and-through suture was easily and rapidly applied. The suturing always began at the mesenterial border and extended upward to the first *double suspension loop;* then the second suspension loop was taken by the assistant, the double loops were drawn taut, and the second division of the intestinal ends were united; having arrived at the second double loop, the suturing was continued down the third division to the mesenterium to the point of commencement. In the large intestine a second row of sutures was added. Finally, the hæmostats were removed, the free ends of the double loops were drawn taut and tied, lending additional support.

The appended cases were done by this method, and have been described in a contribution to the American Medical Association, "Neoplasms of the Colon." They bear the same numbers as in the above dissertation.

CASE 6. Abdominal section disclosed an enlarged uterus; irregular, slightly nodular fundus. The descending colon was prolapsed, and its lower end firmly adhered to the posterior and left side of the uterus, together with an old pyosalpinx. The sigmoid was crowded down into the cul-de-sac, forming several reverse coils. The adherent coil was separated with the scissors and surrounded with gauze swabs and pushed aside. The uterus and adnexa were removed *in toto*. Then the adherent portion of the bowel, which was a cicatricial mass, was excised and an end-to-end enterostomy was done, using the double loop supporting-ligature. Nature of constriction, cicatricial.

CASE 14. Incision through left linea semilunaris. Mass easily lifted out of the abdomen, and found to involve the splenic flexure of the colon. This

mass was excised in the usual way. An end-to-end anastomosis was made by the aid of the double loop suspension ligature, and the intestinal ends were joined by the continuous Czerny-Lembert sutures. The abdomen was closed without drainage. Recovery uneventful. The neoplasm was an adenocarcinoma.

CASE 16. Incision in median line below the umbilicus disclosed a mass attached to the hepatic colic flexure, and was drawn down nearly to the right iliac fossa. The omentum had become quite extensively adhered to it, and at first it appeared to be inoperable. However, the omental attachments were gradually severed, and the mass could be brought into the wound. On close examination it appeared as if the mass was chiefly in the lumen of the colon. It was the size of a hen's egg, and appeared to be attached only to the mesenteric border. The growth, together with about five inches of colon, was removed. The ends of the colon were united by a continuous Connell-Cushing suture, aided by the double suspension loop. The neoplasm proved to be a myofibroma springing from the mesenterial side of the colon.

POINTS IN TECHNIQUE OF CLEFT PALATE AND REMOVING SILK LIGATURES [1]

BY AP MORGAN VANCE, M. D., LOUISVILLE, KENTUCKY

For a long time, in cases of cleft palate, I struggled with the different needles which had from time to time been given us by different operators. I had a fair amount of success with all of them, but none was entirely satisfactory. Some four or five years ago, while doing a staphyloplasty in the case of a girl of 14, I happened, on the spur of the moment, to think of and immediately put into use the following idea in the use of a needle, which has since proven very useful in such work. I had long before come to the conclusion that silkworm-gut was the best suture material, and that the difficulty with the other needles was their size, their use resulting in such a large stitch-hole that subsequent infection was almost unavoidable. I at first used a pair of ordinary straight aspirator-needles of small size. Through one I threaded a silkworm-gut double; through another I placed a single strand, after paring the edges to the cleft. I first passed the needle threaded with the double suture, and pushed the loop through afterwards, and opposite to this I passed the needle with the single thread. It was very easy to engage the single suture in the loop and withdraw both needles, the loop pulling the single suture through, leaving it in the proper position to be tied. One needle was

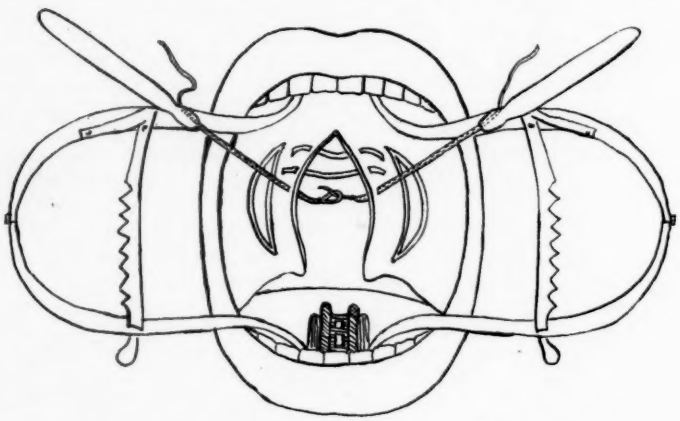


Fig. 1.

[1] Read before the Southern Surgical and Gynecological Association, Baltimore, December 11, 1906.

then threaded with another single thread, and the above procedure repeated until sufficient sutures were in place. When all were tied and the operation completed, the resulting stitch-holes were small, and could be placed exactly where desired.

Fig. 1 illustrates better than I can describe the *modus operandi* of this procedure. It has been a very satisfactory method with me, and I hope some of the fellows will try it.

A second point in technique is the use of a very simple little instrument for removing a silk ligature from a pedicle, wherever it may be situated. I have used it only on the pedicle after nephrectomy, but can see readily how it could be used elsewhere; e. g., in a large size pedicle of an ovarian cyst, where coexisting infection is present or may be expected, or in a vaginal hysterectomy where it is deemed best to use silk ligatures. Some ten years ago I suggested a more elaborate contrivance for this purpose, but failed to have it published, though it was reported and demonstrated. This was the use of a very fine platinum wire, left under the ligature at the time of tying, and brought through to the surface by being attached to a heavier piece of copper wire covered with a rubber draining-tube. When the time came to remove the ligature, an electric current was applied, which instantly heated the platinum wire, thus burning through the ligature. About a year afterwards, Dr. Cleveland of New York thought of and demonstrated the same procedure, and, I believe, put it to practical use.

Some five years ago I was anxious to get rid of a silk ligature on a kidney pedicle, which I feared would be a long time coming away spontaneously, and went to the instrument-case to pick out a long slender pair of scissors in order to remove it, when I noticed an old silver catheter of small size. It immediately occurred to me that I could use it, and this simple little instrument is the result, and it has served me a good turn a number of times since.

I may be behind the times in advocating the use of silk as a ligature material. Some of you may say, "Use catgut, and you will not need the instrument." Well, out in my country a surgeon sleeps better if he knows there is a silk ligature on his case

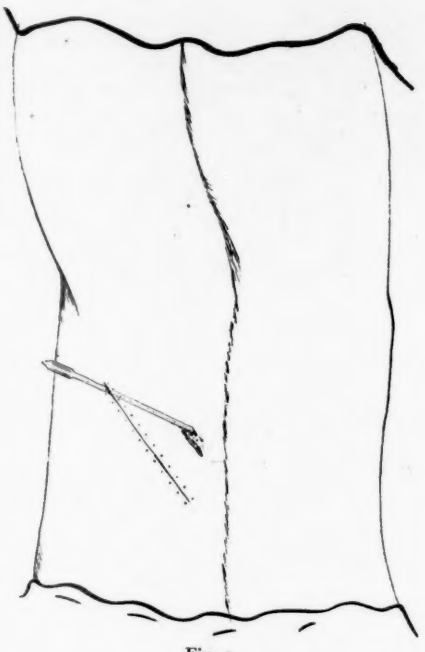
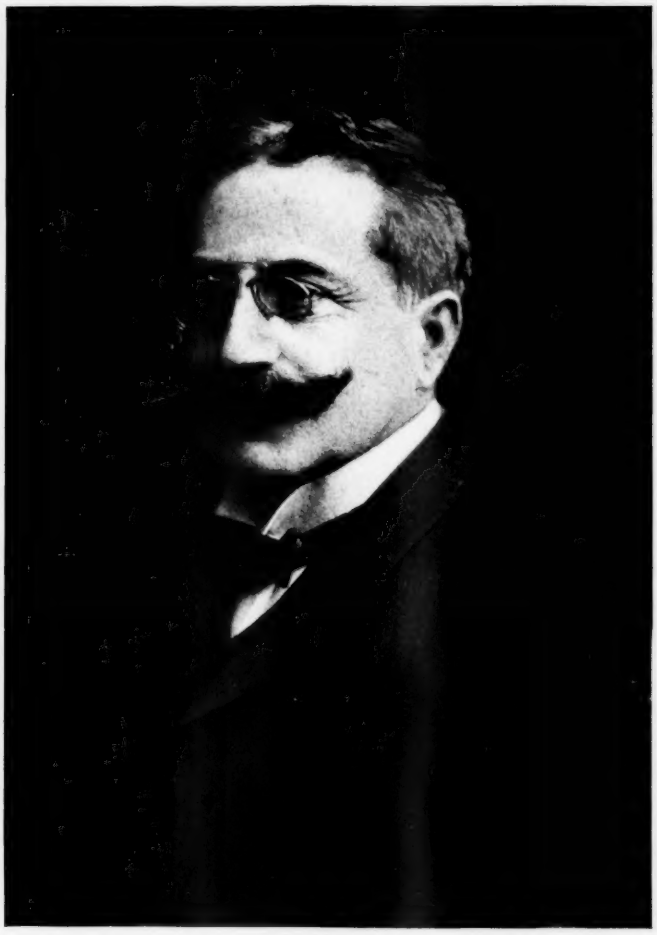


Fig. 2.

of nephrectomy, and as these cases, as a rule, are cases for drainage, and this little contrivance can remove a ligature at any time without inconvenience to the patient, I submit it for your inspection and consideration.

Fig. 2 shows how simple it is, and how easily it does the work. The ligature or ligatures are brought out at the lower end of the wound, and confined there by a plaque of adhesive plaster. At the end of ten days, a silkworm-gut is tied onto the outer end, and is used as a guide to thread the silk through this instrument. The distal end of the instrument is cut in such a way that one side is longer than the other, and this is made sharp. When the ligature is made taut, the knot is drawn up into this tube; then a turn or two cuts it off on one side, without injuring anything adjacent.



HOFRATH DR. A. THEILHAUBER

DEPARTMENT

NICHOLAS SENN FESTSCHRIFT

SYMPTOMS OF VERSIONS AND FLEXIONS OF THE UTERUS

By HOFRATH DR. A. THEILHAUBER, Munich

Translated by E. SAURENHAUSE, M.D., Chicago

THE variety in the external appearance of the creatures of the earth is legion. Among many millions of men, there are not two that resemble each other exactly, with the exception, perhaps, of twins originating from the same ovum. Just as different as the outward appearance are the gait and the language of the different peoples.

It is fair to assume that we should find as many varieties in the texture of the different organs in different men, if in a healthy condition, as we find in their outward appearance, provided our senses had the capacity to notice all of them, and the instruments that aid our senses — the microscope, etc. — were sufficiently improved to enable us to discern the varieties.

Besides, it is highly probable that even the millions of cells that make up man are different in different individuals. We know that in a healthy man two cells, at least, show a different structure in different organs — those of the sperma and the ovum. If they did not display such an immense variety in different beings, the individual progenating from them would not exhibit such a vast degree of heterogeneousness.

It is highly important that those that undertake the diagnosis and the cure of disease shall know at least the varieties existing in the gross structure of the various organs in a healthy individual.

The physician who possesses this knowledge will consider many things pathological that are in fact nothing but deviations from the normal. The female genital organs, especially, even in a healthy condition, vary greatly in their structure, owing to the variety of their physiological functions. The approach of menstruation is accompanied by hyperæmia and œdema of these organs. The uterus and the ovaries increase in volume, shreds of lining are cast off, and other changes take place. When menstruation ceases, hyperæmia disappears.

Pregnancy causes enlargement of the genital organs, the uterus especially increasing greatly in size. Parturition causes many lesions and wounds, forming scars in childbed. The involution of the genital organs, also, increases the connective tissue, etc.

Small wonder, then, that we find many variations of the genital organs, which, however, do not necessarily cause disturbances. I have made a special study of the varieties in structure of the female genital organs. The results of my own work, as well as those of my assistants, have been published mainly in the *Archiv für Gynäkologie* — "Researches concerning the Variety of the Structure of the Mesometrium, Ovaries," etc. The varieties in the structures constituting the uterus and ovaries have not been sufficiently studied up to the present time, but our ideas as to the position of the uterus are, in my estimation, incorrect.

It was recognized at a very early period by physicians that there existed in different women many varieties of the position of the womb. Very likely it was the incarceration of the pregnant uterus, with its sometimes very grave symptoms, which prompted the investigation of the anomalies of the position of this organ. Hippocrates knew the complexity of symptoms of incarceratia uteri gravidis.

It would be misleading to draw conclusions from this pathological condition as to the pathology of retroflexia uteri in a non-pregnant state. In case of incarceratio uteri retroflexi gravidis, it is not so much the malposition which causes the symptoms as the fact that the uterus, being incarcerated in the pelvis, daily increases in size, the other pelvic organs not being able to accommodate themselves to the pressure caused by this tumor, which is constantly growing larger.

We are not allowed to draw conclusions from this condition as to the pathology of the small non-pregnant womb which does not exert any pressure. For a long time the normal position of the uterus was sought. Almost every physician believed that there was only one position of the womb which did not cause discomfort, while abnormalities caused trouble. The opinion as to which was the normal and which the abnormal position varied greatly in short epochs. Bandl (1) illustrates this very strikingly by saying, in his paper read twenty-three years ago before the gynecological section at the meeting of Natural Philosophers and Physicians at Freiburg.

During the last century almost every position of the uterus has been heralded as the normal one, save prolapsus of the womb.

1. Retroversion and marked retroposition were considered normal by Bland in 1838, Tirogaff in 1859, and Claudius in 1865.

2. Anteversion and anteflexion were considered normal, first by the French authors, Boulard and Cusca, in 1853, for the infantile uterus; later, principally by B. S. Schultze.

3. The position of the uterus approximately in the axis of the pelvis was deemed normal. The drawing by Kohlpausch (2) especially, showing the uterus almost in the axis of the pelvis, was very much approved of, and generally accepted by the text-books.

Bandl forgot to mention in his statistics that for a long time the straight position of the uterus was held to be the normal one. In the paper of Sam-

mers (3) we find, for instance, retroflexion defined as a pathological condition of the womb, where the organ does not show a straight line, but where the axis of the upper part forms an angle to the lower one.

Claudius asserted (4) that, under normal conditions, the posterior wall of the uterus in a healthy woman is just as close to the posterior wall of the pelvis as is the lung to the posterior part of the ribs. Some clinicians defended this assertion; Holstein (5), for example. Those physicians who considered a slight bend in the corpus uteri in healthy women a normal condition allowed only a certain angle.

Ronitansky considered a uterus pathological if the angle was less then 165°, which Scanfoni set at 140°.

On account of this view, it was deemed necessary to correct the position in casts which we nowadays consider normal. In thousands of cases, a normal anteversion was changed into a pathological retroversion, and if we accept the reports of the authors, the therapeutic achievements were always good.

These astonishing results suggest that the different positions of the nonpregnant uterus, the normal as well as the pathological, have no effect upon the health of a woman; in other words, that the cause of the prevailing trouble is not to be found in the varieties of the position of the uterus, but in the diseased texture of the womb or in affections of other abdominal organs.

Such a supposition would not be without analogy, as we find in numerous other external and internal parts of the human body great varieties of form and position without any detriment to health.

The heart, for instance, our most important organ, undergoes enormous dislocations after the absorption of a pleural exudation, without any morbid symptoms. In our text-books, there are mentioned as malpositions of the uterus, anteversio and anteflexio, pathologica, lateroflexia, and lateraversio, retroflexia and retroversia.

What symptoms are produced by anteflexion and anteversion? The opinions on this point vary greatly. The leading authors of the middle of the last century believed them to be the cause of backache, abdominal pains, hemorrhage, leucorrhœa, bladder trouble, headache, nervousness, dysmenorrhœa, and sterility.

Then the diagnosis of anteflexion and anteversion was often made. It was made when the body of the uterus could be felt on the anterior vaginal wall. All the troubled symptoms of which the woman complained could now be explained.

The uterus was pushed backward, and was held in this position by various appliances. The patients were cured in a short time, at least according to the text-books of that period on gynecology.

To the untiring efforts of B. L. Schultze it is due that the majority of physicians now accept anteflexion and anteversion, the most common position which has to be considered, as normal, and it does not require any orthopædic treatment.

It would have only been logical, I think, to omit anteflexion and anteversion from the chapter of displacements of the womb. But even Schultze did not want to take such a radical step. Besides the normal anteflexion, he mentions a pathological one, which he calls a malposition of the uterus.

If the uterus, though in a normal position, was restricted in its motility, Schultze called the condition a pathological anteflexion or anteversion. He does not attach any importance to the restricted motility, recommending, as a rule, that only the complicating endometritis, parametritis, etc., should be treated, and that we should not change the position of the uterus.

His propositions were accepted by the majority of gynecologists, after decades of controversy, but a number still cling to the opinion that an anteflexion with a sharp angle must be considered pathological.

This sharp flexion or the restriction of motility are by many considered the cause of morbid symptoms, especially of dysmenorrhœa, sterility, and various forms of bladder trouble. Thousands of experiments, however, made within the last year, on the living body, prove that neither a sharper inflexion at the internal os nor the restricted motility of the uterus produces dysmenorrhœa, sterility, or any other morbid symptoms. Many gynecologists, following the example of Dührssen and Mackenrodt, have placed the retroflected uterus forward, and fixed it as firmly as possible to the vagina, with the twofold result that:

1. The motility of the uterus became restricted much more than we find it in pathological anteflexion and anteversion.

2. The flexion of the uterus upon its anterior walls was much greater than in cases of so-called anteflexia pathologica.

Now, one would expect to find all the symptoms attributed to anteflexion more pregnant. But nothing of the kind happened. Mackenrodt (6) draws the following conclusions from his numerous vaginafixations. No disturbances whatever were found. I can, on account of those brilliant results, recommend the operation most heartily. Physicians placed the utmost confidence in this operation, and the patients are very grateful to me.

Dührssen (7), Rühl (8), and others expressed similar opinions. In fact, the pelvic organs bear the dislocations brought on by vaginafixation and the unnatural bend of the uterus, as a rule, without any disturbances.

If a non-pregnant woman had constant pains after such an operation, they were usually due to an infection during the operation. Publication of the reports of disturbances during labor occasioned by extensive adhesions was the reason why so many German gynecologists abandoned the operation. There being no trouble during labor in cases of non-artificial pathological anteflexion, it is conclusively proven that, from a clinical standpoint, the pathological anteflexion and anteversion are of no importance. I therefore think one would simplify the study of gynecology by simply striking out the chapter on pathological anteflexion.

There was a time when the lateraflexion and version of the uterus were

also treated. We distinguish between dextroversio-flexia and lateroversia-flexia. If the uterus was in a dextraposition, many physicians forced it to the left one by the use of an intrauterine pessary, and *vice versa*.

Nowadays, this practice has been totally abandoned. The lateraflexions are considered by almost all the physicians as unimportant. This being nearly the only point on the deviations of the uterus upon which all agree, I deem it superfluous to mention the reasons why I also think it unnecessary to treat those positions. On the other hand, the explanations of the abdominal pains of many women, attributed to retrodeviations of the womb, play an important part.

The majority of gynecologists think that retroversion and flexion are similar. Permit me to consider jointly the symptoms attributed to them.

As the most constant symptoms caused by retrodeviation of the uterus, Schultze mentions metrorrhagia, abdominal pains, backache, constipation, and leucorrhœa among the less common ones; urinary disturbances, dilatation of the ureters and renal pelvis, hydronephrosis, weakness and paralysis of the lower extremities; besides these, a number of so-called hysterical symptoms; neurosis of the respiratory organs, severe headache, migrane, tubercostal and lumbar abdominal neuralgia, cramps, even epileptic psychic depression, melancholia, and other psychoses.

For a long time I have been of the opinion that all these symptoms are not caused by retrodeviations, but that they are only the result of a metritis or endometritis which does not affect the retroflexed uterus more frequently than the normal one, or they are caused by diseases which have no connection with the genital organs, oftentimes by diseases of the nervous system, hysteria, atonia of the bowels, etc.

If the retrodeviations are the cause of the trouble, we should hear of these complaints more often from women suffering from retrodeviation than from anteflexion of the uterus.

I have compiled statistics from notes made in my practice concerning the frequency of hemorrhagia, metrorrhagia, and leucorrhœa in women suffering from retroflexion of the womb, which I communicated to the seventh Congress der Deutschen Gesellschaft für Gynäkologie.

I could not find a marked difference in the frequency of hemorrhages or leucorrhœa in cases of retroflexion or anteflexion. The assertion that the retroflexion produces a venous stasis, brought on by a retarded circulation in the broad ligaments and the parametrium, causing a congestion of the uterus, leading to metritis, endometritis, etc., and playing an important part in uterine discharge, is probably incorrect, since in most organs of the human body a collateral circulation is established in case of natural or artificial disturbances.

In the uterus especially, the supply and the removal of blood is well provided for, preventing a disturbance in the circulation.

In position, size, shape, and contents, the uterus undergoes more changes than any other organ of the human body. Just think of the great changes

during the eleven months of pregnancy and puerperium. To satisfy all the wants occasioned by the conditions, the uterus is supplied with an enormous net of communicating veins and arteries, so that disturbances in the circulation on account of such slight causes would be impossible. The main drainage-canals for the venous blood are the two pairs of veins, the spermaticæ internæ and the uterinæ, which form wide anastomoses with the venæ uterovaginales and vesicovaginales.

In case of a congestion at any place, the collateral uterovaginal and vesicovaginal veins would become enlarged and carry away the blood. The course of the blood-vessels in the broad ligaments resembles, to a certain extent, that of the intestinal vessels in the mesentery. It is necessary that the blood-vessels of the abdominal organs, in physiologically changing their position, should frequently run in flabby cords, in order to avoid a disturbance in the circulation. The clinical facts, too, speak against a supposition of a venous congestion. Nothing like this was ever seen in the uterus. Let us consider the neighboring rectum, which oftentimes shows symptoms of a venous congestion. There we find, in such cases, the formation of varicose veins and nodules which we call hemorrhoids; but nothing similar is to be found in the uterus. If there were a congestion in the lower half of the uterus, it would be discovered by the use of the speculum. The livid color would disappear after replacing the uterus, and reappear in case of retroflexion.

We oftentimes see the corpus uteri, in cases of retroflexion, complicated by ovarian tumors. I have oftentimes watched this complication when retroflexion was found, but never saw a change of color.

That a collateral circulation is easily established in the uterus is proven by the fact that we never find a wedge-shaped infarct. At least, the most experienced pathologists have assured me that they have never seen one. Schultze attempts to prove his assertion that there is a venous congestion in the uterus by the fact that the uterus, measured with a sound, diminished in size one centimeter and more.

This may be true now and then, but it does not mean much, as the uterus is a hollow muscle, shortening as soon as it contracts.

A flabby heart, also, is larger and wider than when in a contracted condition. By handling the uterus with sound and fingers, it oftentimes contracts. There is another thing requiring consideration:

Lindbloom, in practising abdominal massage, almost always noticed changes in the pelvic organs, especially in the uterus. The changes called increase and decrease by the author do not always appear with the same vigor. Lindbloom thinks they are due mainly to changes in the blood-supply, and he compares them to an erection, the woman, however, not noticing the changes.

As to the result in measuring with a sound, this does not prove much. If one introduce a sound obliquely, with the tip towards the opening of the Fallopian tube, the uterus naturally appears longer than if the sound were inserted exactly in the axis of the womb.

The observations of Winter (9) prove how deceptive the measurements of the uterus with a sound can be. Speaking of the transitions between elongation of the cervix and descension of the whole uterus, he says:

In using a sound, one is liable to be mistaken if he measure up the stretched uterus first and the prolapsed womb afterwards. There is oftentimes a difference of from 4 to 6 cm., which can be explained only by the fact that the cervix is corrugated and that we do not measure up the folds.

A similar difference in length is observed if one measure the elongation of the cervix before the reposition of the uterus and then after. I have found it to vary from 6 to 8 cm.

The rapid involution which one observes a few days after the reposition of the womb is oftentimes explained by the inflexion of the replaced cervix which the sound cannot measure.

The supposition of a retroflexed uterus pressing on the rectum, disturbing its functions and causing a paresis of the limbs, is easily refuted. The rectum is so well protected that this is simply impossible. The same holds good in respect to the nerve trunks in the pelvis, in cases in which a neuralgia or a paresis disappeared after replacing the womb. They were usually nothing but symptoms of hysteria. Here it was only suggestion that cured them.

Any kind of psychic treatment effects a cure, no matter whether it consists in replacing the uterus which happened to be retroflexed or in inserting a harmless tampon, etc.

If the patient has the necessary confidence, a cold shower-bath over the limbs, or a few homœopathic pills, or a pilgrimage, may do wonders.

This opinion I promulgated first in 1895, at the sixth congress of the German gynecological societies. At that time I met with unanimous opposition, especially from Schultze, in an article published in *Monatschrift für Gynäkologie.*

His assertion that hemorrhagia sometimes disappears after the replacement of the uterus is correct, but we should not forget that a good many hemorrhagias cease without treatment. For instance, hemorrhages during change of life, hemorrhages during puberty, many metrorrhagias due to atonia uteri after labor or after miscarriages. There cannot be any doubt that after orthopædic or operative treatment other symptoms of a retroflexio uteri also disappear, but I believe that these facts prove but little. The sojourn in hospital, the rest of mind and body, the absence of the drudgery of daily life — these cure many symptoms.

Schultze admits that besides treating the retroflexion of the womb, it be oftentimes given cold rectal injections, by washing out the uterine cavity, using massage and faradization, the latter being excellent remedies in case of atonia of the bowels, which is oftentimes the real cause of all the symptoms ascribed to reflexio-uteri.

In other cases in which endometritis was at the bottom of the trouble, irrigations of the uterus were very likely to prove efficacious. Soon after the

attacks of Schultze, Jenkins (10) declared that he agreed with me. Then followed, in the *Monatschrift für Geburtshülfe und Gynäkologie*, a short publication by Privatdocent Josephsohn (11), showing that Professor Salin fostered the same opinion in an article published in Swedish in the journal *Hygeia* of London, holding that in case of retroflexio-uteri the pessary, as well as the other means of fixation, was mainly a hypnotic influence.

The assertion of Landau is confirmed in an article from his clinic, by Trendelenburg (12).

Winter (13), from his observations, came to the conclusion that the importance of the retroflexio-uteri was overrated; that only in a few instances the mere misplacement caused the trouble, and the symptoms ascribed to the malposition were due to complications, especially inflammation.

E. Schroeder, interne at the Universitaets Frauenklinik at Königsberg, examined, in the department of internal medicine, 145 patients complaining of symptoms not traceable to the genital organs.

He thinks that the results of the examinations bear out the statements of those who believe that, not the retroflexion of the uterus, but the complications, are the cause of the trouble, and that an uncomplicated retroversion or flexion produces no symptoms whatever. As the main result of his research work he makes the following statement: "The really normal position of the uterus is the anteveresio-flexion."

In women without any symptoms from the genital organs, anteversio-flexion is found in only 75 per cent of all the cases; in 25 per cent we find a retroversio-flexion. Among the latter, two thirds are retroversions, and one half retroflexions.

Consequently, the retroversio-flexion is such a common occurrence, that it is without significance, and does not require any treatment. Kroling and Fenchtwanger (14) examined, in their dispensary, 136 lying-in patients, several months after confinement, recording their complaints.

They received the impression that the woman whose womb was retroflexed did not complain any more than one with an anteflexed womb, and thus the position of the womb was irrelevant. From further observation, these authors drew the conclusion that the retroversio-flexion was not, as heretofore believed, a disease with well-defined symptoms, the same complex symptoms being also found in anteflexion of the uterus.

In many women, hysteria or neurasthenia was found to be the real cause. In many patients operated on in the clinic at Leipzig for retroflexion, it was evident that the malposition did not bring on the pains.

Oftentimes, after the operation, the uterus was in normal position, but the complaints of the patients remained the same, while, on the other hand, the pains ceased in spite of the womb still being retroflexed. The authors also think that in many cases of uncomplicated retroversio-flexia motility, the pains are not due to the misplacement, but to hysteria, neurasthenia, etc. There are no complex or single symptoms characteristic of retroversio-flexia.

Heinrici (15) also published a paper, giving mainly an account of the opinions of gynecologists in Denmark, Sweden, and Norway, showing that in these countries the belief that the retroflexia-uteri is of little account is more widespread than in Germany, owing to the publications of Vedeler and Salin.

Willie (16) says that as regards the retroflexion he more and more accepts the creed of modern teaching.

Baisch (17) reports from the clinic of the University of Tübingen a number of cases in which the retroflexion was changed to an anteflexion by an operation. (Alexander-Adams operation or ventrofixation.)

In repeated examinations the uterus was found in a normal position, but the trouble of the patient remained the same.

Blach (18), in 147 out of 195 cases of retroflexions, found no connection whatever between the misplacement and the symptoms, and came to the conclusion that on account of his observation we have to attach less value to the symptoms of the disease, and that we cannot prove conclusively that the favorable results are due to the corrected position. His paper originated from the clinic of Fischl.

Klein (19) watched the influence of the orthopædic treatment on his patients, having a retroflexed uterus, and found that only in a very limited number of cases the changes and fixations of the position of the womb bore any influence upon the health of his patients, and said that thereafter he would employ orthopædic treatment only in cases of incarceratio-utero gravidi retroflexi prolapse, sterility, etc.

Pfannenstiel (20) declares Schultze's dogma, as far as the retroflexion is concerned, a heresy. The retroversio-flexion itself is not to be considered pathological, makes no uterine symptoms, and therefore it is a mistake to treat an uncomplicated case of retroflexion locally. The same opinion is expressed by Mather (21), first assistant to Prof. Dr. Krauer, Graz,) in his paper on enteroptosis: "The retroflexion itself is irrelevant; only the complications cause trouble."

Dr. Lucy Waite (22) of Chicago comes to the following conclusions:

1. The normal position of the uterus is characterized by its absolute motility. A uterus not affected by chronic metritis, and freely movable, may assume any position in the pelvis without causing disturbances.

2. If you find a retroflexion with pelvic symptoms, it is necessary to search for complications as the cause of the symptoms.

3. It is therefore necessary to treat the symptoms and not the retrodeviation.

Every year more people adhere to my teachings. The longer they adhere, the more they are convinced that an organ like the uterus, suspended in the middle, between two organs hourly changing their positions, may assume various positions without affecting the health of the bearer.

Every position of the uterus within the true pelvis is normal; abnormal is the position of the uterus outside of the pelvis — the prolapse of the uterus.

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TRANSACTIONS OF SOCIETIES

TRANSACTIONS OF THE SOUTHERN SURGICAL AND GYNECOLOGICAL ASSOCIATION

The nineteenth annual meeting of the Association was held at Baltimore, Maryland, December 11–13, 1906.

The Association met at the Hotel Belvedere, with the President, DR. GEORGE H. NOBLE of Atlanta, Georgia, in the chair.

An address of welcome was made by DR. HOWARD A. KELLY.

SULPHATE OF SPARTEIN IN SURGICAL PRACTICE

DR. STUART MCGUIRE of Richmond, Virginia, said he believed he had accidentally discovered in this drug a valuable remedy for the prevention and treatment of post-operative suppression of urine. In the last five years he had lost more cases from post-operative suppression of urine than from all other causes combined, and this, despite the almost routine use of chloroform as an anæsthetic. The cases had usually been those with pre-existing nephritis from sepsis or cholæmia. Shock had not apparently been a factor, as the condition would not develop for twenty-four or thirty-six hours. A patient operated on for retention of urine or for jaundice due to obstruction of the common duct would do well for one or two days, and just as the patient was thought to be out of danger there would come the news that he was passing no urine. He would become restless, then listless, would develop a stupor which would rapidly deepen into coma, and would die with all the symptoms characteristic of uræmia. In the treatment of this condition he had tried water by mouth, under the skin, and in the rectum; hot packs and vapor baths; cups and counter-irritants; strychnia, digitalis, and nitroglycerine; calomel and saline purgatives, and in one case stripping the kidney capsules — all with uniformly bad results. Two years ago he began empirically the use of sulphate of spartein, and he now had a record of six cases in which he was sure the drug was the means of saving the patient's life. Its therapeutic effect was to increase the blood-pressure, make the pulse slower and stronger, and act as a powerful diuretic. Its action was manifest in thirty minutes after administration, and lasted for four or six hours. To get results, sulphate of spartein must be given hypodermically in from one to two grains, repeated every three to six hours. Its use should not be delayed until suppression of urine existed, but it should be prescribed as a prophylactic, as well as a curative agent. He did not claim that it was a specific, or that it should be employed to the exclusion of other measures, such as purgatives, transfusions, and hot packs. He did believe, however, from actual experience that it was preferable to the drugs of the digitalis type in rapidity of action, ease of administration, and efficiency of results.

DISCUSSION

DR. WILLIAM PERRIN NICOLSON of Atlanta, Georgia, had used sulphate of spartein in a way similar to that of the essayist. He preferred it to digitalis as a heart tonic. Therapeutically, it combined the virtues of being a powerful heart tonic and of dilating the capillaries. An important point was to give it in large doses — say, two grains. Usually, it was put up by drug manufacturers in doses of one tenth of a grain. Its value depended on large doses. He recalled one case in which suppression of urine lasted for forty-seven hours, but the patient finally recovered.

THE COMPARATIVE ADVANTAGES AND DISADVANTAGES OF HYSTERECTOMY AND REMOVAL OF THE BODY OF THE UTERUS

DR. J. WESLEY BOVÉE of Washington, D. C., read a paper with this title. (See paper, *ante*, p. 268.)

DISCUSSION

DR. ISAAC S. STONE of Washington, D. C., had never regretted having left the cervix. So far as malignant disease was concerned, it had been his good fortune to save two patients by subsequent operations, who had what was called subtotal hysterectomy done, with stump remaining, which was subsequently found to be malignant, and operation was done. Both of these patients were still alive. However, one was a case of sarcoma. He thought a large number of surgeons were well satisfied with the mortality of the Baer operation of leaving the stump.

DR. HENRY T. BYFORD of Chicago said that no

cut-and-dried rule could be laid down to take out the cervix in the class of cases under discussion. One would not think of removing the cervix in a young woman unless there was demonstrable malignant disease. Even in an old woman, it was a dangerous procedure, and one attended with great difficulty. The cervix should not be removed unless there were good reasons for so doing.

Dr. Maurice H. Richardson of Boston recalled but one instance in which cancer of the cervix developed after supravaginal amputation for fibroids. While he had not looked up his records, he should say that cancer of the cervix was very rare after such operations.

Dr. Herman J. Boldt of New York said that so far as malignant disease in the cervix after supravaginal hysterectomy was concerned, it must be admitted that it did occur. He did not believe, however, that if the cervix was left it was a causal factor in the production of malignant disease. So far as the relative risk between supravaginal amputation and panhysterectomy was concerned, he did not consider that it cut any figure. One who was accustomed to doing pelvic surgery could do one operation as quickly as the other. The main question was, What was the condition of the vaginal vault subsequently? In the majority of instances it made no difference whether there was a good vaginal vault following supravaginal amputation, or whether there was a vaginal vault with more or less cicatricial tissue in it, but it did occasionally make a difference, for he had seen patients become markedly neurasthenic as the result of scar tissue in the vaginal vault, and he did not consider it desirable to do a panhysterectomy when the cervix was in a fairly normal position. When the vaginal portion of the cervix was diseased, the vagina torn, and the cervix eroded, the cervix should be removed; but otherwise he thought it was just as well to leave it, because it made a better pelvic floor.

Dr. Robert S. Hill of Montgomery, Alabama, said there were as potent reasons for leaving the cervix in some cases as there were for removing it in others. If we considered the anatomy of the pelvic floor, we must conclude that the removal of the cervix weakened the pelvic diaphragm, and therefore post-operative vaginal hernia was more likely to occur. The verdict of the majority of the profession was that panhysterectomy was an operation of greater magnitude than supravaginal amputation of the uterus, when we considered the increased traumatism to the parts, the increased liability to injury of the rectum and bladder, and the increased chance of interfering with the ureters through cicatricial tissue, as well as the increased chance of infection by entering the vagina. He could not agree with the statement that panhysterectomy, as an operation, was of no greater magnitude than was supravaginal amputation of the uterus. This might be true in the hands of the most expert hysterectomists. But we must consider what could be done in the performance of hysterectomy by the average surgeon, and not what could be done by men of superior skill. When the subject was considered from this standpoint, then it would seem to the speaker that supravaginal amputation of the uterus still had a place in the category of legitimate surgical procedures. As to the amputation of the body of the uterus increasing the chances of cancer of the cervix, he was not aware of any evidence that supported that position.

EXTRAUTERINE PREGNANCY

Dr. O. L. Shivers of Marion, Alabama, reported two cases of extrauterine pregnancy.

AN IMPROVED AND ACCURATE METHOD OF LOCATING FOREIGN BODIES WITH THE X-RAY

Dr. Robert Carothers of Cincinnati, Ohio, said the most important step in foreign body surgery was some definite idea of the location of the foreign body. The X-ray had been of untold value in developing this step. Stereoscopy, as applied to radiography, called stereoskiagraphy, was the most improved method, and would seem to give the most accurate location of a foreign body. This idea had been worked up by many radiographers in different parts of the country, independently of each other. The ordinary X-ray plate looked flat. The stereoscope gave a perspective view very much as if the parts (bone and bullet) had been one in nature. Two separate plates were made of the same part without changing in the least the position of the part skiagraphed, which was placed on the compression-diaphragm table, the compression diaphragm itself being discarded, on a hollow board or plate-holder, with a metallic bottom, so that the rays might be partially obstructed, and in this way one plate could be removed and a second supplied. The tube was held in the tube-holder, and with the use of a plumb-bob it was carefully centered through the center of the part to be skiagraphed onto the center of the plate. The plates were made with the tube placed $1\frac{3}{8}$ inches to either side of this central point, so that they were made from points $2\frac{3}{4}$ inches separated from each other, corresponding to the distance between the human eyes. The developed plates were placed in highly illumined boxes facing each other, and were from them reflected into two small looking-glasses, which were placed at a right

angle to each other, with the apex of the angle placed between the eyes of the observer in the stereoscope. One now looked into the two looking-glasses; each eye, in a separate glass, saw a separate plate; then by adjusting the plates and the stereoscope to get an exact focus, the two plates were made to look as one, and it would appear that there was one looking-glass behind which the observer looked, reflecting the object back onto the retina, producing a perspective view. This method was of especial value in locating foreign bodies in the spine, thorax, and abdomen, where it was almost impossible to get a plate which was not flat by any other method.

THE SURGICAL TREATMENT OF THYROID DISEASE, BASED ON THREE HUNDRED PERSONAL OBSERVATIONS

Dr. Charles H. Mayo of Rochester, Minnesota, said the medical profession of America was taking a progressive position in recommending operation as a cure for goiter, including also exophthalmic goiter. The simple goiters in girls at puberty were not included in this class. Excepting operations for malignancy and Graves's disease, the great majority of operations were those of expediency and not of necessity. Therefore a low operative risk was imperative. In uncomplicated cases the mortality following operations was almost an accidental one. There were 110 cases of Graves's disease, with 9 deaths, and but 2 of these were in the last 64 operations. In two sarcomas and six carcinomas there was one operative death, which occurred on the third day. Aside from those recorded, there was one death from pneumonia, on the eighth day. Pathologically, there was a change in the gland, which was essential to Graves's disease or hyperthyroidism. This was in the greater number, size, and shape of the cells, and in the lack of normal colloid.

Patients were prepared as to lung complications, and the rhythm and tension of the pulse, which should be carefully investigated, as they were the danger signals.

Very rarely was local anæsthesia employed, the preference being for the open drop method of giving ether. The patients received from $\frac{1}{120}$ to $\frac{1}{150}$ grain of atropine, and $\frac{1}{6}$ of a grain of morphine, twenty to thirty minutes before the administration of the anæsthetic.

The position of the patient was head up (reverse Trendelenburg), with a roll of gauze under the neck to elevate small tumors. A transverse collar incision was made through the skin and platysma muscle, the sternohyoid muscles being separated at the midline. In exophthalmic goiter and complicated cases the anterior muscles, on one side at least, were severed as high as possible over the thyroid to preserve the nerve-supply and break the scar effect. Cysts and encapsulated growths were enucleated. Other goiters were extirpated, care being exercised in preserving the parathyroids to prevent tetany and in protecting the recurrent laryngeal nerve. In this, one was aided by leaving the posterior capsule of the gland in the extirpation. Sufficient gland substance should be preserved to prevent myxœdema, and, as it depended upon the character of the gland, no definite rule could be given as to what constituted the proper amount.

DISCUSSION

Dr. George W. Crile of Cleveland, Ohio, said that in a series of 114 operations upon the thyroid gland, his smaller experience had compared very well with the conclusions reached by Dr. Mayo, with the exception that he had been unable thus far to get as low a mortality as was reported by Dr. Mayo from operations in the presence of hyperthyroidism. In the late cases of colloid goiter of long standing, if symptoms of Graves's disease appeared, he had found that operations on this class were comparatively simple and safe. Young persons with acute Graves's disease, coming on without previous goiter apparently, appearing with other symptoms, he had found to be great risks, from an operative standpoint. Of the 114 operations upon the gland, there were 5 cases of carcinoma and 5 of sarcoma. In one of the cases of carcinoma the author noticed unmistakable symptoms of Graves's disease. He had not had a fatality in any case of Graves's disease engrafted upon a chronic goiter, but he had had three deaths in twenty cases of acute Graves's disease following operations.

Dr. Randolph Winslow of Baltimore said he had had the opportunity of seeing some surgical work on the thyroid gland done abroad this summer at the Kocher clinic and elsewhere, and had been particularly interested in the anæsthetic employed in these cases. Personally, he had been using scopolamine-morphine anæsthesia by injection as a means of general anæsthesia, and the infiltration of the skin with the Schleich solution for strictly local anæsthesia, and found that it worked well, and that, as a rule, the patients did not complain.

TOXÆMIA OF PREGNANCY

Dr. W. M. Jordan of Birmingham, Alabama, called attention to the non-specific nature of the liver lesions in cases of toxæmia of pregnancy, and said that much the same character of changes occurred in toxæmias from other causes. There was a decided tendency for toxæmia cases to get

worse after the uterus was emptied, except in the case of eclampsia, which, for special reasons, was not considered in the paper. The cause of this behavior of toxæmia cases was attributed to a probable increase in the pre-existing toxæmia, through the addition of toxins resulting from the process of involution of the uterus. For this reason, pregnancy should be terminated at an earlier date than would otherwise seem necessary, as this probable post-partum increase in the toxæmia must be allowed for. Attention was called to the probable unfavorable influence of anæsthesia, as most of the inhalation anæsthetics were to some extent liver poisons.

Speaking of the treatment, the writer expressed himself as being a firm believer in radicalism. He advised that pregnancy be terminated as soon as it became evident that the symptoms were of toxæmic origin. The reasons given by him were the impracticability of influencing the antitoxic function of the liver, and the unreliability of treatment directed against the toxæmia itself.

Dr. D. F. Talley of Birmingham read a paper in which he reported eight cases of toxæmia of pregnancy.

CHOLECYSTECTOMY — WHEN INDICATED — SOME OPERATIVE SEQUELÆ

Dr. I. S. Stone of Washington, D. C., said that cholecystectomy had become popular because cholecystotomy, or rather cholecysthotomy, had frequently failed to cure patients with gall-stone disease. He preferred the designation as just stated, because the mere finding and removal of gall-stones frequently failed to cure many of the invalids who had pain, jaundice, or other symptoms of some mischief about or within the biliary passages. Some operators spoke of dilatation of the common and hepatic ducts as a result of cholecystectomy, and Robson was convinced that this dilatation was a compensatory and perhaps a conservatory action, although he had found that stones developed in such dilated duct as they did in the gall-bladder. Cholecystectomy sometimes failed to prevent a recurrence of colics and of other symptoms which necessitated operation. An important question, therefore, naturally arose, Under what circumstances should the gall-bladder be removed? This question could not be answered until the possibility of definite knowledge of the hepatic and common ducts was considered, especially their patency and contents, and if they were the seat of an infection.

A careful review of the recent literature convinced the author that, however popular this operation had become, the bad results had not as yet been reported as generally as would appear desirable. Nearly all of those who favored cholecystectomy as an operation of choice agreed to operate when the gall-bladder had, by reason of trauma, infection, or malignant disease, become either useless or dangerous. They say, however, that the operation should add very little to the risk involved in a cholecystotomy. The Mayos removed two out of every five gall-bladders they inspected. They performed a cholecystectomy under the following conditions: When stones were lodged in the cystic duct, the removal of which would leave permanent interference with drainage; when the disease was confined to the gall-bladder, and when it was filled with pus or any fluid or bile which had undergone septic change; when the gall-bladder had become greatly thickened or had lost its function, or was the seat of inflammation without stone in which the liver ducts were not involved, or in malignant disease.

Nearly all authors who had recorded their experience practically agreed with the Mayos.

The mortality of cholecystectomy had been compared to that of cholecystotomy. The writer thought, however, that very few would succeed in obtaining the low rate reported by the Mayos, who had a record of 1.37 per cent for cholecystotomy, and 1.64 per cent in cholecystectomy. The author thought the two operations could not be compared for safety in the hands of most operators. Cholecystectomy would be performed by all surgeons for conditions which were a result of severe, long-standing disease which had caused any complications besides the changes in the gall-bladder itself. On the other hand, cholecystotomy was one of the least dangerous operations in abdominal surgery when the patient had only gall-bladder disease, or possibly a stone with healthy bile. Therefore the author thought that Robson's figures — 57 operations, 33 of which were of gall-stones complicated by various morbid states, with a recovery in 32, and 13 of which were for fistula — were without signification, unless compared with precisely similar cases in other hands. The same might be said of 72 cholecystectomies collected by Klieber, with a mortality of 16, of which 7 were due to the operation.

The contraindications to cholecystectomy were not numerous, but were of great importance. All the authors agreed upon one point; namely, the propriety of leaving the gall-bladder for drainage purposes in pancreatitis, especially the chronic interstitial variety. The same opinion was expressed by most authors regarding the propriety of leaving a gall-bladder when a cholecystenterostomy might be needed, as in certain common-duct obstructions.

The writer's experience with cholecystectomy had been satisfactory in some particulars, and he found it especially useful in those cases where the gall-bladder had been out of use for a long time. In contracted bladder with an open choledochus, in obstructions of the cystic duct, whether due to stone or other causes, which have resulted in permanent alteration of either bladder or duct, he found the very best indications which would assure perfect cure. In cholelithiasis he had had two unfortunate results, because stones continued to pass at intervals after the removal of the gall-bladder. He knew of a third case, in the practice of another surgeon, where the same difficulty occurred.

Cholecystectomy requires a better and clearer knowledge of the hepatic ducts than we have hitherto possessed, or the operation will fall into ill favor. The demand at the present time is for a safe and practical method of insuring patients against subsequent attacks of pain, jaundice, or any kind of distress after gall-stone operations.

DISCUSSION

Dr. Robert T. Morris of New York said that, in the past, surgeons groped over the abdominal wall in the dark, and it was only recently that they had touched the button and turned on the light. He thought that surgeons could turn the light on this subject if they included all gall-stone and cholecystitis cases under one heading, and called them cholecystitis cases and adhesion cases. Adhesions in the region of the gall-bladder and bile-ducts would produce the same symptoms as gall-stones, whether gall-stones were present or not. He cited a case in point on which he had operated. Proper selection of cases was everything in surgery. With increased experience and further observation, he inclined more and more toward the complete removal of the gall-bladder, for the reason that there was a chronic infection going on all the while in many patients.

Dr. W. P. Carr of Washington, D. C., thought surgeons should consider these cases more as instances of cholecystitis and the gall-stones as incidental, when it was remembered that gall-stones did not cause any symptoms until they produced cholecystitis or obstruction of the common bile-duct. It was really a cholecystitis or an obstruction of the bile-ducts that led to the diagnosis of gall-stones. The gall-bladder should be left unless there were positive indications for its removal.

Dr. Samuel Lloyd of New York said the possibility of an occlusion of the duodenal opening in gall-stone cases was not infrequent. He had operated once for congenital closure of the opening from the ampulla of Vater into the duodenum. In a number of gall-stone cases, surgeons found a very much distended common duct, that is, the duct was distinctly larger than usual, and one should suspect occlusion at the ampulla of Vater, the stone coming down and acting as a ball-valve, pushing against the duodenal opening, producing an inflammatory condition, and finally contraction. The only hope of possible recovery of patients with this type of trouble was to do a choledochoduodenostomy. He spoke of inspissation of the bile throughout the ducts in some cases.

Dr. Henry O. Marcy of Boston referred to the early history of surgery of the gall-bladder and gall-ducts, and said he had written a good deal on this subject. If any one would take the time to look up the early literature, he would find that Marion Sims, in his time, went over the ground as thoughtfully and carefully as any one could do it to-day. Eighteen years ago, Dr. Marcy operated on a woman for the removal of a gall-stone in the common duct. A few weeks ago he operated on the same patient for the purpose of releasing adhesions which had caused biliary obstruction. The woman recovered from the operation.

Dr. J. Montgomery Baldy of Philadelphia said that in many cases a diagnosis of gall-stones or of gall-bladder disease was made, yet at the operation no gall-stone or disease was found. No adhesion, no cancer, and no occlusion was found; yet the patient had undoubted symptoms, so far as one could tell, of long-standing gall-stone trouble. On the other hand, in other cases, operations had disclosed disease of a gross character of the gall-bladder; the patient had been relieved, apparently, but after leaving the hospital there was a return of the symptoms, even after a secondary operation for cholecystectomy. He recalled one case of long standing of that character. He admitted that in this entire field the problems were as yet very largely unsolved, and so far as his own practice was concerned, there were still elements of doubt, both diagnostically and pathologically.

Dr. Joseph Price of Philadelphia reported four recent cases, and said that in two, when he opened the abdomen, he had to side-step to get out of the way of the filth of gall-stones. The patients were dying, and he was called to see and operate on them because they were dying. They were not suitable for the removal of the gall-bladder. He thought if he had attempted to remove the gall-bladder in these cases he would have lost the patients on the table. He made toilets, drained, put in coffer-dams, and the patients were now well.

Dr. Frank Martin of Baltimore, by invitation, called attention to two cases of cholecystectomy

which he reported in 1895. Since that time he had not changed his opinion, although he had not operated on many cases since. He had resorted to cholecystectomy in fourteen cases out of sixty. In most of the fourteen cases there was a chronic cholecystitis, with disorganized gall-bladder and acute gangrenous infection. In two or three of them there was an acute gangrenous inflammation of the gall-bladder. In these, cholecystectomy was followed by excellent results. He had had no deaths or serious shock following the operation.

Dr. Howard A. Kelly of Baltimore said that when he operated for any intra-abdominal pathological condition, it was now his practice to examine the gall-bladder, among other organs, to determine whether it was diseased or whether gall-stones were present or not. He believed in cholecystectomy where the gall-bladder was grossly diseased, although one could not always do an ideal operation. He recalled two cases in which an ideal operation could not be done, on account of the great density of the adhesions.

Dr. Charles H. Mayo of Rochester, Minnesota, said that, in their clinic, they occasionally operated, after having made a thorough examination, thinking they had a case of gall-stones to deal with, and found no stones. It was rare, however, for them to diagnose gall-stones or disease of the bile-ducts, even though no stones were found, without finding some abdominal condition which would have made it necessary to operate. He did not think a patient cared whether he had gall-stones, duodenal ulcer, or ulcer of the pylorus, if he could be relieved. In other words, the patient had no special preference, although he might think it nice to have the gall-stones in a bottle to show to his relatives and friends, proving that he had had that trouble. Some years ago they operated and found a blue gall-bladder, which was ordinarily considered a healthy gall-bladder, but it was covered with adhesions. They drained such gall-bladders and tried to relieve the adhesions. Two of these cases, however, were again operated on, one a year afterwards, the other two years thereafter, for the same condition, and at that time they found that a duodenal ulcer was the cause, which had been overlooked at the first operation.

THE TREATMENT OF HEMORRHAGE BY DIRECT TRANSFUSION OF SIMILAR BLOOD

Dr. George W. Crile of Cleveland, Ohio, said the basis for the clinical use of transfusion of blood in the treatment of hemorrhage was as follows: That normal blood of an individual of one species was physiologically equivalent to that of any other individual of the same species, and was therefore interchangeable; that the vascular systems of two individuals might, by an end-to-end anastomosis of vessels, be united; that such union might be accomplished so as to approximate intima to intima; that the blood that was transferred across this artificial continuity did not come in contact with any foreign body or any tissue, but the intima; that, under these conditions, blood would not clot, and the rate of flow and the amount transferred were under immediate control. Only the gravest and apparently hopeless cases had thus far been transfused. Altogether, fourteen clinical cases had been observed. All were done painlessly, under morphine and cocaine anæsthesia. No unfavorable symptoms of importance had been noted. The donors had regained their lost blood in from five to six days. Every donee had felt immediate improvement. Striking evidence of this had been shown in the facies, the improved circulation, and the feeling of well-being and even buoyancy. The technique was by no means simple, and must be carried out with absolute precision, to prevent clotting. In no instance was there observed nephritis, hæmoglobinuria, agglutination, hæmolysis, or laking of blood. The transfused blood apparently exercised no unfavorable influence on its new host, and the latter none upon the new blood. The blood of individuals of the same species, therefore, seemed to be physiologically interchangeable. Certain cases of pathological hemorrhage had been cured by the addition of fresh normal coagulable blood. Transfusion in a case of pernicious anæmia was tried, with totally negative results. Partly on the basis of Wright's work on opsonins and partly on other grounds, two cases of transfusion after bleeding in tuberculosis had been done. In every instance, in the research and in the clinic, the effect of hemorrhage had been overcome by the replacement of the lost blood by means of transfusion. He was not yet ready, however, to reach conclusions or to make deductions or predictions. This was but a report of progress.

THE MANAGEMENT OF LAPAROTOMY PATIENTS, AND THEIR MODIFIED AFTER-TREATMENT

Dr. Herman J. Boldt of New York City, in a paper on this subject, said that no particular preparatory treatment was necessary for patients upon whom it was intended to do an abdominal operation, unless the operation involved the opening of the stomach or the bowels. Stomach lavage was of benefit at the conclusion of the operation. Patients should not be kept unnecessarily under an anæsthetic. The application of a tight bandage around the upper part of the thighs, to keep a blood-reservoir in the lower extremities, in exsan-

guinated and very weak patients, was excellent. The same might, in exceptional cases, be done with one of the upper extremities. These bandages were taken off as soon as the operation was completed, and thus more blood was thrown into the trunk. The administration of strychnine during and after an operation should be made with more care than is usual. The intravenous infusion of a 0.9 per cent saline solution should not be too long delayed when the condition of the patient made it evident that its employment might be of benefit. In instances of large myomata, where the patient is much exsanguinated by hemorrhage, it is desirable that the infusion be begun as soon as the patient is fully under an anæsthetic, so that by the time the operation is completed, about 1,000 to 1,500 c.c. may have been infused.

The application of a very simple dressing was made over the wound, and also the adjustment of a snugly fitting Scultetus bandage, made of oxide of zinc plaster. The administration of a dose of morphine was desirable if there was restlessness or pain, the medication acting clinically as a heart stimulant. The author allowed regular diet and unrestricted mobility within twenty-four hours after the operation, unless specially contraindicated. Patients were allowed to get out of bed as soon as possible after an operation. The author avoided forced catharsis before the first four or five days after an operation, unless there was a special indication for it.

In cases where resort to vaginal drainage was had, or where it was evident that there would be some secretions intraperitoneally after an operation — as in purulent cases and oozing from torn adhesions — trunk elevation was employed as soon as the patient was in bed. For this the employment of a bed-lifter, which the author exhibited and described, or the placing of high blocks or chairs under the head of the bed, was preferable to the use of back-rests.

THE TREATMENT OF SENILE GANGRENE

Dr. Edward H. Ochsner of Chicago called attention to the fact that there were two distinct types of senile gangrene, the first occurring in cases with only moderately generalized arteriosclerosis, but in which there was a distinct mural thrombus. In this class of cases there was an early formation of a distinct line of demarcation, and it was best to delay operation until this line of demarcation had become well established, and then to amputate directly through the line of demarcation.

In the second class of cases there was a marked generalized arteriosclerosis, the whole affected extremity was markedly involved, there was no tendency to the formation of a line of demarcation, and an early high amputation was indicated.

PUERPERAL THROMBOPHLEBITIS OF THE PELVIC VEINS

This was the title of the President's address, delivered by Dr. George H. Noble of Atlanta, Georgia. Attention was directed to the surgical aspects of this question. Of the 32 cases of puerperal sepsis collected, the infection was divided as follows, with reference to location: Metrophlebitis, 11; metrolymphangitis, 4; abscess in parametrium, 8; thrombophlebitis (suppurating) of broad ligament, 2; suppurative peritonitis, 7; and deep laceration of vagina, 7. Thus, of the entire number, there were 13 cases of pelvic phlebitis, two of which were in the large veins of the pelvis. If thrombosis of the pelvic veins occurred in six or seven per cent of puerperal infection, the mortality being high, the author thought considerable saving of life might be brought about by early recognition of the disease and prompt interference. He thought the showing should be greater, for the number of cases coming under his observation did not correspond with the percentage given in the above list.

Thrombosis might begin early, but usually the symptoms were not clearly defined until late in the puerperal state, making a distinction between it and other clinical manifestations of sepsis, excepting intramural abscess of the uterus. But, exceptionally, the symptoms might appear earlier. In one case the chill came on on the third day, and on the ninth the thrombus had advanced to such an extent that resection of the left side of the uterus and of the corresponding broad ligament was required. The disease was often complicated with other septic conditions, such as involvement of the tubes, ovaries, suppuration in the parenchyma of the uterus, parauterine tissues, or the clot itself might be permeated with pus, the latter escaping directly into the circulation. In differentiating between thrombosis and intramural abscess of the uterus, uncomplicated by peritonitis and adhesions, small, rounded, flat elevations on the surface of the uterus might be felt in patients with very thin or relaxed abdominal walls. Such cases presented the ordinary septic pulse and temperature curves, and did not have the distinct interval of non-suppurating thrombus. When complicated by adhesions or diseases of the appendages, with tense abdominal walls, exclusion of intramural abscess and detection of tortuous veins were extremely difficult.

Suppurating thrombosis, very likely, might be overlooked when complicated with peritonitis,

pus-tubes, etc., unless the symptoms of pyæmia supervened, or the pulse showed a tendency to become climbing in character. These were suggestive of the true condition. Any puerperal case with pelvic lesions, variable temperature, and climbing pulse of three to four weeks' duration, without signs of improvement, justified an operation of some kind, especially if the uterus proved negative as the source of trouble. If a mistake was made in diagnosis, and the location of the infection was found in the Fallopian tubes or abscesses in other parts of the pelvis, no surgical error was committed, for they, too, were in need of serious attention.

There were several plans of procedure: 1. Excision of the thrombosed veins; 2. Ligation of the thrombosed veins; 3. Hysterectomy. In addition to these, attention should be given to the complicating conditions.

One author objected to excision, on account of the liability of reinfecting the wound by soiling the parts with the clots.

In two of the author's cases in which the veins were lacerated by digging the clots out of the broad ligament with the fingers, the temperature range for two or three days was quite irregular, and reached high marks at short intervals (103°–105°), but some of this temperature might have been due to the extensive surface involved. Each of these had numerous abscesses in the uterus, ovaries, and pus collections between the coils of the intestines; besides, the omentum and intestines were softened and adherent from the umbilicus down. As the chill was not repeated in either case, it was likely that soiling the wound with clots had little to do with it. The number of cases in which excision had been resorted to was too small from which to draw accurate conclusions. Ligation of the veins was not satisfactory, especially if the clots became infected with pyogenic cocci. Hysterectomy was extremely dangerous in this condition. In the first place it was often delayed too long to remove successfully all the infected centers. When thrombosed vessels were confined to the uterus, there were some chances of success, but propagation through the vessels into the broad ligament extended the infected field into the pelvic veins, and at times the uterus might not be at fault, for the thrombi might lie exclusively in vessels outside of that organ. Removal of the uterus under such circumstances was worse than useless, as it would leave the thrombosed vessels undisturbed. On the other hand, excision of the diseased veins without hysterectomy was promising, except in extreme cases.

In the summary of the small number of cases accessible, the results were as follows: Resection of vein, a mortality of 28⅔ per cent; ligation of veins, 44⅘ per cent; hysterectomy, 64¼ per cent.

He ventured the assertion that until further experience shall have worked out the solution of this problem, the opinion must be accepted that early recognition of septic thrombosis of the pelvic veins, followed by prompt excision, was the best method of relief we could offer our patients in this disease.

SURGICAL TREATMENT OF TUBERCULOSIS OF THE KIDNEY

Dr. Howard A. Kelly of Baltimore read a paper with the title. (See paper, *ante*, p. 257.)

TUBERCULOSIS OF THE KIDNEY

Dr. Charles P. Noble of Philadelphia read a paper on this subject. (See paper, *ante*, p. 264.)

A STUDY OF VAGINAL CÆSAREAN SECTION, WITH ESPECIAL REFERENCE TO SUBSEQUENT PREGNANCY AND LABOR

Dr. John F. Moran of Washington, D. C., read a paper on this subject. (See paper, *ante*, p. 337.)

DYSTOCIA FOLLOWING FIXATION AND SUSPENSION OF THE RETROFLEXED UTERUS

Dr. J. Whitridge Williams of Baltimore, in a paper on this subject, reported 4 cases in his own experience, and collected 34 from the literature, in which Cæsarean section had to be done following some method of fixation for suspension of the uterus.

Dr. Henry O. Marcy of Boston gave a brief sketch of the life of one of Baltimore's greatest men — Dr. Horatio Gates Jameson, whose greatest contribution to surgery, he said, was the occlusion of arteries by buried animal ligature.

HODGKIN'S DISEASE — A TYPE OF SARCOMA

Dr. William B. Coley of New York referred to the fact that, at present, both pathologists and clinicians were widely at variance as to what constituted Hodgkin's disease. The theory that Hodgkin's disease was a form of tuberculosis, strongly advocated, a few years ago, by Sternberg and others, has been practically disproved by the work of Dorothy Reed. The theory which, at the present time, received the greatest support, both by pathologists and clinicians, was, that Hodgkin's disease was a separate entity, in the nature of an infectious process. This was the position held by Reed and the majority of recent writers. The only other theory that had received much consideration was that Hodgkin's disease was in the nature of a malig-

nant tumor, or a variety of sarcoma. While this was the view originally held by Virchow, who called it lymphosarcoma, and Billroth, malignant lymphoma, the same had been practically abandoned by later writers, and only recently had again begun to receive serious consideration. In favor of the infectious theory of the origin of the disease, Reed cited the facts: 1. That fever was frequently associated with Hodgkin's disease; 2. That the frequency with which the disease started in the cervical region suggested the possibility of infection gaining entrance through the mucous membrane of the mouth; 3. The absence of capsular infiltration and implication of adjacent structures; 4. The peculiar giant cells found in the tumors; 5. Finally, and most important of all, the fact that metastases occurred only in pre-existing lymphoid tissue.

Dr. Coley, who strongly believes in the malignant nature of Hodgkin's disease, in answer to these objections, stated that his observation had been that fever was not an infrequent occurrence in malignant disease, especially in sarcoma, if generalization had taken place.

As to the neck being the frequent starting-point of Hodgkin's disease, suggesting the possibility of infection, he stated that the same reasoning would apply to cases of sarcoma of the neck which bore such a large proportion to the total number of cases. Coley had observed 70 cases of sarcoma of the neck in a total of 615 cases.

As to the absence of capsular infiltration, this was by no means a universal characteristic of the tumors of Hodgkin's disease, as shown by the recent and very valuable contribution of Gibbon of San Francisco, whose report covered nine cases. His cases showed marked evidence of capsular infiltration, and also answered a more important objection: that metastasis occurred only in pre-existing lymphoid tissue. In some of Gibbon's cases, metastases occurred where there was no possibility of the presence of lymphoid tissue, and not only infiltrated the capsule, but the surrounding structures also, even the periosteum and bone.

As another reason for regarding the process as of the nature of a sarcoma, Coley cited several cases in which the clinical diagnosis was sarcoma, while the pathological report read Hodgkin's disease. Cases of Hodgkin's disease pursued a clinical course identical with that of sarcoma, infiltrating the surrounding tissue and causing death in precisely the same way.

The recent work of Banti went far towards proving that not only Hodgkin's disease, but also leucæmia, both lymphatic and myelogenous, were really forms of sarcomatosis.

Coley's conclusions, based upon a study of upwards of six hundred cases of sarcoma, of which seventy originated in the lymph-glands of the neck, are, that Hodgkin's disease was merely a special variety of sarcoma, representing a fairly definite clinical and pathological type, but in many cases shading off into other types that corresponded most closely with that of an ordinary round-celled sarcoma.

The evidence brought forward by Reed and others in favor of the infectious origin of Hodgkin's disease need not be disproved. Most of it held true in the case of sarcoma, and all of it but added to the slowly accumulating mass of evidence in favor of the infectious or microparasitic nature of sarcoma, a conviction which the writer had long held.

CYSTS OF THE OMENTUM

Dr. R. E. Fort of Nashville, Tennessee, read a paper on this subject, in which, after referring to the literature, he reported the following case:

Female, aged 2½ years, was presented February 21, 1906. Was fairly well nourished, and gave a history of no illness, except two attacks of acute indigestion, each lasting several days. Abdominal enlargement was noticed eighteen months before by mother, which progressed until the child had abdominal circumference of twenty-eight inches at umbilicus. General health and nourishment had been very good, with no symptoms referable to the abdominal condition, except some dyspnœa upon exercise.

Examination revealed the following physical signs: Great abdominal distention; abdominal veins prominent; complete dullness on percussion over the entire abdomen. Not a resonant note could be found anywhere. Fluctuation and a decided thrill wave could be elicited at any portion of the abdomen. Pulse 100; temperature and urine normal. A reserve diagnosis of ascites or ovarian cyst was made.

The enormous abdominal distention led the author to the conclusion that gradual relief of the hydrostatic pressure upon the splanchnic vessels offered the safer course. Consequently, on February 23d, five pints of dark bloody fluid was removed by aspiration. In five days, examination showed some decrease in the size of the abdomen from the time of the aspiration.

The abdomen was opened through a median incision, extending from above the umbilicus upward. There presented at once a dark glistening tumor, having the appearance of a distended gangrenous intestine, which, owing to its flaccid condition, was delivered through the incision without rupture. It was found to be a collection of fluid in the folds of the greater omentum, extending from

under the pylorus on the right along the greater curvature of stomach, to and including the folds of the gastrosplenic omentum. A chain ligature of catgut was applied from right to left, including the omentum and the gastrosplenic ligament. There were no adhesions, so the work was rapidly accomplished. The abdomen was closed, and the child left the hospital, well, in a few weeks.

Closer observation of the specimen shows the absence of a distinct capsule, the fluid being between the folds of the omentum. There was an absence of fat in the omentum, but the vascularity of the omental wall was great. Closer inspection showed that the stab of the aspirator five days before had not closed, but continued to leak. This accounted for the decrease in the abdominal measurements from the day of aspiration to the day of operation. There was no free fluid in the peritoneal cavity, the absorption being quite in proportion to the leakage. The fluid had a specific gravity of 1007, contained albumin and many degenerated blood-cells.

In reviewing the literature of the subject, the author found that the case which he had reported made the twenty-second one.

The three points which impressed him most were: 1. The impossibility of diagnosis without exploration; 2. The condition was seen most often in children under ten years, 50 per cent under ten, 65 per cent under twenty, which led to the belief that it was of congenital origin; and 3. It occurred more frequently in females (75 per cent).

THE SURGICAL ASPECTS OF GASTRIC CARCINOMA

Dr. John B. Deaver of Philadelphia read a paper with this title. (See editorial, p. 98, January, 1907, number of Surgery, Gynecology and Obstetrics.

VAGINAL SECTION AS AN OPERATION OF CHOICE

Dr. Henry T. Byford of Chicago read a paper with this title. (See paper, *ante*, p. 262.)

Dr. John C. Munro of Boston reported an unsuccessful case of aneurysmorrhaphy for abdominal aneurysm.

APPENDICITIS IN THE NEGRO

Dr. Hubert A. Royster of Raleigh, North Carolina, said that in order to obtain some information on this subject, he wrote to several surgeons practising in Southern cities having a large proportion of negro inhabitants, and the replies were unanimous in regard to the rarity of the disease in this race. During the ten years in which the author had practised, he had removed the diseased appendix from negro patients fifty-four times. In forty of the cases a pathological appendix was found while operating for other conditions, such as pus-tubes, fibroids, etc. Besides a few of these which contained pus, over one half of the others were filled with fæcal matter, usually soft. Practically none of these cases presented a definite history of appendicitis, but only that of the original lesion. Fourteen cases of primary appendicitis in the negro were operated on, three in the first seven and a half years, and eleven in the last two and a half years. During the latter period — two and a half years — the total number of appendix operations on both whites and negroes was 123, giving a relative frequency of practically nine per cent in the colored race.

INJURIES AND DISEASES OF THE HYOID BONE

Dr. Randolph Winslow of Baltimore reported six cases — one that had come under his own observation and in his practice, and five from the literature. He said that tumors of the hyoid bone were both malignant and innocent in about equal proportion, and both required thorough removal, with as much of the contiguous tissue as might be necessary. An early operation was as much to be desired in neoplasms of this bone as in those occurring in other parts of the body. The author's case was one of sarcoma of the hyoid bone and larynx. He excised the tumor of the hyoid and base of the tongue, larynx, and part of the pharynx, under local anæsthesia. The patient recovered from the operation, but died two months later, from metastasis. The patient was a man, forty-five years of age, who entered the hospital on January 9, 1906, suffering from dyspnœa due to a growth in the region of the hyoid bone on the left side, which obstructed the larynx. Thirteen years ago a lump was removed from the hyoid bone and the side of the larynx, and patient was discharged cured, in six days. It began to recur in three years, and had been in existence for ten years, slowly increasing in size. The voice was altered; there was dyspnœa, with stridor, cough, dysphagia; pulse rapid and weak. There was a lump found in the region of the hyoid bone, projecting more to the left than to the right side; it was globular, and as large as a goose-egg, hard and movable, but not painful. A skiagraph failed to show the hyoid bone, but a shadow of a growth was seen between the jaw and the larynx. On January 11th, tracheotomy, under cocaine, was done to relieve the urgent dyspnœa. Patient improved somewhat and desired a removal of the growth. It was thought inadvisable to administer an anæsthetic; hence morphine-scopolamine was injected hypodermically and the skin infiltrated with Schleich's solu-

tion. Operation on February 1, 1906. Hyoid bone, base of tongue, whole larynx, and part of pharynx were excised without difficulty, while the patient was conscious. The trachea was attached to the skin at the lower part of the neck and a large tracheal tube introduced; pharynx and skin sutured. There was no shock. At first the patient was fed through a large soft catheter passed from the mouth into the œsophagus; but later the pharynx opened and a tube was passed from the neck into the œsophagus. Patient did well, and soon sat up and was able to walk about the ward; but recurrence occurred in the neck, and he gradually failed, and died in eight weeks. An autopsy showed extensive metastasis in the lungs, bronchial glands, liver, and mesenteric glands, whilst the microscopic examination of the growth showed it to be a round-celled sarcoma.

The following papers were also read:

CONSERVATIVE METHOD OF MANAGING UNDESCENDED TESTICLE, by DR. JOSEPH PRICE of Philadelphia.

GALL-STONES WITHOUT SYMPTOMS, AND SYMPTOMS WITHOUT GALL-STONES, by DR. MAURICE H. RICHARDSON of Boston.

POINTS IN TECHNIQUE OF CLEFT PALATE AND REMOVING SILK LIGATURES, by DR. AP MORGAN VANCE of Louisville, Kentucky. (See paper, *ante*, p. 343.)

REPORT OF A CASE OF RUPTURED OVARIAN TUMOR WITH COMPLICATIONS, by DR. RUFUS B. HALL of Cincinnati.

SOME DISJOINTED OBSERVATIONS UPON OUR TECHNIQUE, by DR. A. MORGAN CARTLEDGE, Louisville.

INFLUENCE OF RESPIRATION UPON DEVELOPMENT OF THE CHEST DEFORMITY IN SCOLIOSIS, by DR. MICHAEL HOKE of Atlanta.

SOME SUGGESTIONS IN REGARD TO THE SURGICAL TREATMENT OF EMPYEMA, by DR. SAMUEL LLOYD of New York.

NORMAL INVOLUTION OF THE APPENDIX, by DR. ROBERT T. MORRIS of New York.

SPLENECTOMY, INDICATIONS AND RESULTS; REPORT OF FIVE SUCCESSFUL CASES, by DR. W. P. CARR of Washington, D. C.

OFFICERS

The following officers were elected for the ensuing year: President, Dr. Howard A. Kelly of Baltimore, Maryland; Vice-Presidents, Dr. R. E. Fort of Nashville, Tennessee, and Dr. Hubert A. Royster, Raleigh, North Carolina; Secretary, Dr. W. D. Haggard, of Nashville, Tennessee, re-elected; Treasurer, Dr. Charles M. Rosser of Dallas, Texas, re-elected.

New Orleans, Louisiana, was selected as the place for holding the next meeting, on Tuesday, November 12, 1907.

TRANSACTIONS OF THE CHICAGO GYNECOLOGICAL SOCIETY

FRANK W. LYNCH, EDITOR

A regular meeting was held December 21, 1906, with the President, DR. JUNIUS C. HOAG, in the chair.

The PRESIDENT stated that a committee had been appointed to draft suitable and appropriate resolutions relative to the death of Dr. Fernand Henrotin. This committee consisted of Drs. E. C. Dudley, Charles B. Reed, and T. J. Watkins. The committee was now ready to make its report.

DR. REED then read the following:

Whereas, A strong man has fallen in the van of battle; and

Whereas, His comrades, though realizing as yet very imperfectly the measure of their misfortune, yet, in recognition of his sterling qualities as a man and physician, do nevertheless desire to give outward expression to the sorrow which his death has occasioned; and

Whereas, In special convocation it is unanimously felt that the Chicago Gynecological Society, and the members thereof, have sustained a grievous blow in the loss of their fellow, Dr. Fernand Henrotin, who, by his energy, integrity, and singleness of purpose, had achieved high honor in the medical profession, even as his warm-hearted, affectionate disposition and nobility of character had won him high place in the hearts of his friends; therefore, be it

Resolved, That, in appreciation of his noble life, and as a mark of sorrow for his untimely death, this inadequate expression of esteem and affection be spread upon the minutes of the Society, and, as a token of sympathy and commiseration, a copy of the same be transmitted to the bereaved family.

On motion of DR. C. S. BACON, the resolutions were unanimously adopted by a rising vote.

PLACENTAL POLYP

DR. LESTER E. FRANKENTHAL: I meant to have here sections of a specimen which I removed this morning, but, unfortunately, we could not get the sections ready.

The patient was 49 years old. I saw her two days ago, when she applied to me for treatment on account of hemorrhage. She told me she thought herself ten weeks pregnant, and that she had miscarried. On account of her age, I deemed it advisable to dilate the uterus by the tent method, in order to make a digital exploration and see whether there were retained secundines, or a carcinoma of the body of the uterus. I removed what seemed to me to be a placental polyp. Dr. Herzog could not get the sections ready for to-night, but I believe he concurred in my opinion from the macroscopic appearance of the specimen. This certainly is a late pregnancy, the patient being 49 years old.

PEDUNCULATED FIBROID

The second specimen is from a patient upon whom a hysterectomy was done, on account of this large fibroid, and I present the specimen on account of two things. First, we are constantly overlooking a careful history of the patient. This woman, from her early girlhood, was compelled to rise two and four times a night to urinate; and when the case came to operation and the peritoneal cavity was opened, the tumor was still covered by visceral peritoneum. I found a pedunculated fibroid with a broad base, that had partially twisted on its axis, so that the bladder was situated high on the left side and somewhat to the rear of the tumor — a most unlikely place in which to find the bladder. I spoke of it at the time, suspecting the unusual site of the bladder. I did not wish, however, to pass a sound into the bladder, on account of the fear that we might infect the patient in some way, and after having made the usual incision across the tumor and stripped back the peritoneum with gauze, I unfortunately tore the bladder in two places. I at once proceeded to sew the injury to the bladder before continuing to enucleate the tumor, sewing one tear transversely, and the other one in the opposite direction, longitudinally, so that one line of sutures would not make traction upon the other. After having enucleated the tumor, I attached the remnants of the uterine body to the anterior abdominal wall, in order to give an additional hold to the bladder suture. The woman made an uninterrupted recovery. We might well have suspected the bladder to be involved, had we more carefully studied the well-written history; and secondly, I never before encountered a bladder to the side and somewhat behind a fibroid of the uterus.

TWISTED OVARIAN TUMOR

The next case is that of a twisted ovarian tumor. The patient was operated on a few days ago; and the diagnosis was made on account of symptoms of ileus and the presence of a fluctuating tumor which could be plainly felt above the pubes. It was easy to diagnose, by rectal examination, the position of the uterus as being behind and to the left. It is needless to discuss this case any further. Her recovery was uneventful.

The next case I should like to present is one of which Dr. Herzog will present the sections.

A few months ago, before my summer vacation, I showed before the Society a specimen of what seemed to be a deciduoma malignum. I did a hysterectomy on that patient. Since that time I have seen two more cases of myxomatous degeneration of the chorion, making three in a short space of time, and in each instance there developed within the uterine cavity a tumor similar in character to the one that developed in the uterus which was shown before the Society in September. One of the cases was not in my service, but belonged to Dr. Banga.

Now, in the case we have under discussion to-night, I asked the woman to return to the hospital after an absence of five or six weeks, on account of hemorrhages. I dilated the uterus again with tents, and found this mass in the uterus, which, to the sense of touch, is identical with the one that was seen here at the meeting in September, and which was diagnosed as deciduoma malignum, and I believe the diagnosis was concurred in at that time by Drs. Ries and Schmauch, who were kind enough to examine the specimens. In this last case Dr. Herzog finds simply inflammatory granulation tissue, and on that account I have sent the patient home, and have not done a hysterectomy. The question that arose in my mind was, whether we should be governed by the microscopical findings in these cases, or whether we should do a hysterectomy; whether we should rely on the microscopical findings, in spite of the fact that there is a good-sized mass in the uterine cavity. The woman is forty years of age, and it may not be amiss to say that the function of the uterus is practically terminated, and the question arose, whether it would be wise to wait for developments, and possibly lose the woman's life, or whether we should do a hysterectomy. On that account, I should like to call on Dr. Herzog, and have him tell us what he has found microscopically, and likewise get opinions from members, if I may.

DR. M. HERZOG (by invitation): When Dr. Frankenthal first removed the hydatid mass, I made a number of sections, and, as a result of my

study, came to the conclusion that there was nothing suggestive of malignancy. I believe that those of you who have had large experience in studying the human placenta will agree with me that neither the macroscopic nor the microscopic appearance of the tumor suggests a malignant process. The first section shows swollen chorionic villi bounded by normal Langhans and syncytial layers. There are syncytial buds as well as islands of proliferated Langhans cells; also islands of decidual cells.

A few weeks later, Dr. Frankenthal removed other pieces of tissue from the uterus, and in the examination of these I found uterine mucosa, some regenerated uterine glands, and in some areas, groups of large cells which I believe to be decidual. However, in some other parts there are large cells containing large nuclei, most of which are in a condition of degeneration, and which contain irregular masses of chromatin. My opinion is this: There is nothing in the sections which would warrant a diagnosis of chorio-epithelioma, because there is no evidence of syncytial masses which penetrate deeply into the uterine musculature. I believe the large cells are decidual, but, of course, you know it is sometimes exceedingly difficult to decide whether the degenerating parts of a placenta are composed of Langhans or decidual cells. My opinion is, that a diagnosis of chorio-epithelioma cannot be made from the sections. If, however, on physical examination, it is found that this woman has a uterine tumor, I should advise that the uterus be removed, because of her age and the condition of a growing tumor.

I should like to have Dr. Webster and Dr. Schmauch, as well as those who are interested in human placentation, to examine these sections, and give their opinions as to whether they permit of a diagnosis of chorio-epithelioma.

Dr. George Schmauch: Some time ago I read before this Society a paper on the differential diagnosis between the malignant and the simple epithelial proliferations of chorionic tissue, but I fear that it has aroused but very little interest.

As regards the specimens presented by Dr. Frankenthal, I should like to ask whether there has been a recurrence of the growth in this case.

Dr. Frankenthal: No.

Dr. Schmauch: At the September meeting of the Society I did not give as my opinion that the case then reported by Dr. Frankenthal was absolutely a malignant chorio-epithelioma.

Dr. Frankenthal: I did not say you did, but thought you had given that opinion.

Dr. Schmauch: At that time I asked the Doctor for a specimen from which I might prepare sections, but did not receive one. The sections of his specimen that were of especial interest to me showed a collection of lymphatic cells around the chorio-epithelial formation. These findings have not yet been described. I have observed them before, but am not able to say whether they have any influence on the prognosis in chorio-epithelial proliferations. My opinion is, that there is only one type of chorio-epithelioma which is malignant in the sense that we regard carcinoma as malignant, and that is comprised of both syncytial and Langhans cells. Besides this form, there are transition forms, in which there is a predominance of the large syncytial cells. This type is less malignant, but must be classed with the chorio-epithelioma malignum. There is also a third form — the atypical chorio-epithelioma — consisting of large cells without admixture of syncytium. This is perfectly harmless. I believe that we have no right to speak of a malignant growth so long as the chorio-epithelial proliferations remain in connection with the villi. It is the duty of the clinician to decide whether he is dealing with generalized or localized growths, as this point cannot be definitely determined by the pathologist.

There are cases in which a perfectly normal mucosa is presented, and yet the tumor has given metastases to different regions of the body. I believe, therefore, that we have no right to make a diagnosis merely from the scrapings.

We must bear in mind that the time that has elapsed since pregnancy may also be of importance in our diagnosis. A mole is rarely expelled completely, and therefore we should expect to find villi in uterine scrapings some weeks after the abortion. Many of the cases which are described as malignant chorio-epithelioma are merely degenerated hydatid moles, or else placental polyps. Such cases recover after operation, because we are dealing merely with a localized growth, in which the villi have not degenerated beyond the depth of the muscle. I showed in my paper that forty per cent of the cases which were incorrectly described as chorio-epithelioma malignum, and which recovered following operation, were merely local growths, in which villi were found in the curettement preceding the operation.

Dr. J. Clarence Webster: I think the specimen is a very interesting one. It is certainly not surprising in a case of hydatid mole to find an appearance similar to that presented under the first microscope, apart from conditions which we call syncytioma. There is, however, in this specimen, very little degeneration, and that, I think, is important. Degeneration in the syncytium, in Langhans layer, and in the stroma of the villi of retained portions of the placenta takes place at varying

periods after an abortion or labor. The appearance of the stroma of the villi in this specimen is that of fresh tissue, as if it were from a very recent pregnancy. I think that must be borne in mind as a significant factor. It is perfectly true that we find a whole series of variations, from a simple hydatidiform post-partum growth to the characteristic malignant form, and I do not think any one is prepared to state just when we are in a position to conclude as to malignancy. I have always been inclined to place stress on the question of degeneration in such a mass retained in utero. After a pregnancy, degeneration of either epithelial or connective-tissue elements indicates to me non-activity, and does not suggest growth. A specimen like this, found in a uterus several weeks after labor, with undegenerated villi well distributed through the mass, would make me feel very anxious about the case.

DR. FRANKENTHAL (closing the discussion): I wish to thank the members for their remarks, and to say, in answer to Dr. Schmauch, that if we wait for the clinical picture of malignant deciduoma, the patient will surely die, even if operated on. This woman had constant hemorrhages for six weeks. I did not wait for her to expectorate blood and to have metastatic growths in the vagina and other parts of the body, but I wanted to know what I might or should do as a gynecologist with this uterus. She had hemorrhages constantly and profusely, and I felt that the diagnosis of the pathologist, in which I had supreme faith, was correct, and accordingly sent the woman home. On the other hand, if she were a member of my family, I think I should have extirpated the uterus, as then I should have been willing to take the responsibility upon myself. The mass was irregular, while in the other case I presented it was smooth. It is about an inch long by one half or three quarters of an inch in diameter, and the upper part of it presents a distinct separation from the lower mass. It probably reaches from the internal os one half to three quarters the length of the body to the fundus, and is situated on the posterior wall.

DR. E. C. DUDLEY: I may have misunderstood Dr. Frankenthal when he said that if the patient were a member of his own family he would rather have the uterus removed, but did not care to assume the responsibility of taking out the uterus when the patient was a member of some other family.

DR. FRANKENTHAL: That is practically what I said. I would rather assume the responsibility of taking out the uterus of a member of my own family than that assumed in the case of a member of some other man's family, when the pathologist's and the clinician's diagnoses are at variance, thus guarding against the possibility of a subsequent malignancy. However, I did not feel like removing the uterus in this case, because we know that in some cases in which there is myxomatous degeneration and subsequent proliferation of tissue left behind after the removal of the mole, recovery may take place. Of course, the disease may go on and terminate in malignancy. I would not attempt to remove the uterus of a stranger under just these circumstances, unless I had the support of members of this Society.

PRIMARY OPERATIONS FOR OBSTETRICAL DEBILITY

DR. EDWARD REYNOLDS of Boston read, by invitation, a paper with this title. (See paper, *ante*, p. 306.)

DISCUSSION

DR. F. S. NEWELL of Boston, by invitation, opened the discussion. He said: Mr. President, I have been very glad to hear Dr. Reynolds's paper to-night, because it expresses views which I have held myself for some time. It is a significant paper, coming from perhaps the most conservative medical community in the country, and as an expression of opinion from Boston it should not be accepted, but it is the expression of one man's opinion, and of the opinion of a few men who have been following in his footsteps, rather than in the footsteps of the general profession; it expresses our views, although in some respects the author goes further than we are prepared to go.

Before taking up the paper, I wish to go a little more extensively into the history of one of the patients of whom Dr. Reynolds spoke. I took care of her in her third and fourth pregnancies. Her third pregnancy was fairly satisfactory for the first six months, but for the last three months she went progressively backward. Apparently no one could control her under the conditions in which she was then placed. To keep her away from household cares and from domestic matters seemed almost impossible. I decided to wait as long as her general nervous condition seemed fair, and to interfere at the first sign of nervous breakdown, which came approximately at the date of labor; dilatation was perfectly easy, and, as Dr. Reynolds has said, there was no spasmodic contraction of the uterus, because the pains had not come on. The whole operation, manual dilatation and the application of high forceps, occupied half an hour, and she made a satisfactory recovery.

In her fourth pregnancy she came to me in an extremely nervous condition, due to worry over the illness of her third child. The conditions at home having changed, I was able to put her on an absolute rest cure; sleeping out-of-doors and living

out-of-doors. She had no household cares, and there was everything to make life easy. This time she came to labor in a much more improved condition than that of her third pregnancy, and vastly better than in the beginning of the fourth pregnancy. I decided to apply the test of labor. The nurse was to call me as soon as labor was present, and, although the patient was within six miles of my office, I only reached the house in time to catch the baby. The whole labor lasted three hours. The patient made an uninterrupted convalescence. This case emphasizes what proper care and proper conditions mean in pregnancy. It is impossible to give this care to all women, because of conditions at home, so that we cannot hope to reach the ideal in all cases.

Taking the paper as a whole, one thing that has impressed me ever since I left the medical school is this, that obstetrics is one branch of medicine which is guided by tradition, and that we are constantly caring for our patients by this or that method because that has been the custom in years gone by. It is time for obstetrics to be considered as a progressive branch of the science of medicine, and its general tone raised. I think one reason why tradition, and particularly tradition against early operation, has entered into obstetrics is because, in the past, obstetrics has been in the hands of medical men and the expectant treatment has been carefully carried out. Obstetrics will never be properly done until the obstetrician is a surgeon, and prepared to take surgical risks and to make obstetrics a surgical subject. Until then, I do not think we can make any definite advance. Obstetrics must be a definite specialty, and must be done by men who are doing little else; otherwise women will not receive proper care during pregnancy, or the care during labor which they demand. The conditions of life have changed in the last few years in all large cities. At present, the women who come to labor are not the natural normal women of whom our text-books speak, and of whom descriptions of labor and operations are written. Of the women who come to labor, a large proportion of the civilized women have abnormally developed nervous systems, and they react to a stimulus which the natural woman would not respond to. The result is, if we let the civilized woman go through a long labor we often find great nervous exhaustion. She may or may not stand the strain satisfactorily. I have seen in consultation, in five years, ten or twelve cases which went through an easy delivery, either easy low operative deliveries or normal deliveries, in which sudden collapse occurred within two or three hours after labor and death seemed imminent. The symptoms might have been those of hemorrhage, but there was no external hemorrhage. There was a weak pulse, sweating, cold extremities, pallor, and air-hunger. The most probable explanation that I have been able to give for the majority of these cases is that a weak heart has given way; not an organically diseased, but a flabby, weak heart, which has been overtired during labor, and we have a condition of acute dilatation; in the other cases it seems as if there were hemorrhage into the abdominal vessels, and, owing to changes in the abdominal pressure and to nerve stimulation, particularly stimulation of the abdominal nerves, the woman bleeds internally. These patients seldom die; but, in my experience, the number of cases is constantly increasing, showing that women are going to labor in a worse and worse condition. There is an increasing class of cases in the community, the extreme cases of which Dr. Reynolds has just described, who are unfit to undergo labor. The cases he means are those in whom labor is evidently a risk, and these cases can usually be judged by the rules which he has applied; the debilitated, flabby women, with unsatisfactory eliminative power and unsatisfactory nervous condition, whose general condition runs down during pregnancy. These cases, unquestionably, are better surgical risks than obstetrical risks. There are other cases on the border-line. In these we can afford to apply the test of labor for a few hours. If we find that the progress made, as compared with the work still to be done, has set up an exaggerated train of symptoms, that woman is a subject for immediate delivery, irrespective of the stage labor has reached. I have repeatedly been sorry that I have not operated earlier. I have never been sorry that I have operated as early as I have. The advantage of operating on cases of the class Dr. Reynolds has spoken of before labor begins is apparent, since, to my mind, if we allow labor to go on for some hours, say even three or four hours, we often find spastic contraction of the uterus, and increased difficulty in dilatation and delivery, while my experience in operating before the beginning of labor, except in eclampsia, when the uterine tissues are always rigid, has been that dilatation is comparatively easy and extraction safe. The choice of operation, whether the operation be done from below or a Cæsarean section, should depend on the condition of the soft parts as well as the pelvic indications. If the soft parts are easily dilatable, the operation from below, in the absence of other indications, is favorable, and yet I believe that in these cases there is much more shock and more risk of damage in the operation from below than in the abdominal delivery. In cases in which there is rigidity of the soft parts, and extensive laceration

is practically sure to take place, there is no question but that abdominal delivery is the best for the patient, irrespective of the pelvic condition. Shock is less, and convalescence after abdominal operation more rapid, with very slight complications, from an obstetrical standpoint.

There is one point Dr. Reynolds spoke of to which I wish to take exception. He speaks of tentative traction with forceps when in doubt as to the best method of delivery. Personally, I do not like to put forceps into the uterus when I am in doubt, and may decide to perform Cæsarean section. A test that I have applied personally, instead of the tentative use of forceps, is to grasp the head with one hand above the pelvis, and with the other hand in the vagina, to see how far the head could be forced into the pelvis. If there is a marked disproportion in the head, I would not hesitate to do Cæsarean section; but if the head can be forced into the pelvis to any extent, I would try the use of forceps. That test is satisfactory, on the whole.

I am very glad to have heard the paper, because it is a distinct advance in obstetrics. We need men who are willing to operate on these cases when both patients are in good condition, instead of deferring operation until one is dead and the other dying. No one would prefer to operate on a case of general peritonitis when he might have removed the appendix in the early stages of inflammation or between the attacks, and it seems to me that obstetrical operations should now be placed in the same category with surgical operations, and that our rules should be the same.

Dr. Joseph B. De Lee: I have no doubt that a physician practising in a community of intelligent people, when his practice, such as has been laid down to-night, becomes generally known, will become suddenly popular with the women, as a great many of them will simulate neurasthenia and other asthenic conditions to enjoy the pleasures of rapid delivery without pain. Nevertheless, I believe that there is a great deal to commend in the paper, and, I am firmly convinced, a great deal to criticise unfavorably. The general trend of the paper is excellent. The first thing that impressed me was the importance the writer attaches to ordinary obstetrical cases, and those of you who are familiar with what I have said in various meetings will thus hear, from Boston, a confirmation of the importance I have always ascribed to an ordinary normal obstetrical case. The ordinary, so-called normal obstetrical case, in my experience, is a great rarity. I cannot call a function that has distinct dangers to the mother and baby normal, nor can I call a function that is always attended with lacerations of the cervix, with some stretching, and laceration of the levator ani, an important muscle, laceration and stretching of the pelvic fascia, laceration and stretching of the perineum, displacement of the rectum, resulting in a train of symptoms, a normal one. Nor can I call a function normal that has a mortality of three to five per cent for infants, that maims a large number of infants, by spastic paralysis, spinal paralysis, cerebral palsies, various forms of peripheral palsies, of which Erb's paralysis of the arm is an example, and that is attended with infection of the mother and of the babe. In fact, I could go on for half an hour enumerating the accidents of so-called normal labor. I confess, I have seen very few perfectly normal labors. That the woman has gotten through alive, and the baby has gotten through alive, are not criteria of a normal labor. There is no other function that is attended with such mortality and morbidity, and the other functions are called normal. I have made the statement repeatedly, that, except in a woman in perfect health, labor is not a normal process. Viewed from that point, then, I shall be seen as confirming the opinions of the gentleman who read the paper and of the one who discussed it. But there is a shadow side to the question. Is it possible to determine beforehand, in a given case, whether a woman is going to have an easy or a hard labor, or one that is going to take a great deal of her strength and leave her a neurasthenic afterwards? I think, in a great many cases, it is; in a not inconsiderable number it is not. Can we adopt, as a rule, in these cases of debility, an early operation, that is, the abdominal or internal operation? Before discussing that point, I should tell my plan in such cases. It is a modification of the one given by the essayist, and I seek to terminate labor as soon as possible after dilatation of the cervix is completed, and the resistance offered by the cervix overcome; I do not allow the woman to have a prolonged second stage and become exhausted, with her bearing-down efforts, but terminate the case as soon as possible with forceps. I believe the essayist errs when he says that delivery by means of rapid dilatation and forceps is an operation without danger. I think it increases the danger for the baby by the production of cerebral injuries with the forceps, and we have substituted for the cerebral injuries of natural labor the cerebral injuries produced by forceps. I believe, too, that it is not so easy in a large number of cases to dilate the cervix so readily and with such safety as the writer has said. I cannot dilate cervices so easily. They tear in my hands. Harris of Paterson, New Jersey, dilates cervices in this way in a very short time, and says that he has never seen or had a cervix that he could

not dilate. He told me that these cervices do not tear in his hands by the use of his instrument. I have dilated cervices with the Bossi dilator, with an instrument approved by Bossi, following his instructions, and yet I have torn these cervices. Other men in this city dilate cervices without tearing them, and dilate them rapidly. I cannot do it. The cervices I dilate, when closed tightly to start with, tear, into the broad ligament sometimes. I therefore have high respect for the cervix. In the cases cited, if I had to deliver from below, I should prefer a clean-cut vaginal operation, like a vaginal Cæsarean section, where I can cut as far as I want, and then sew up nicely and neatly. In rapid delivery, if the perineum is unprepared, the levator ani may tear in undesirable directions, unless cutting is resorted to. Therefore you have to add to the operation of dilatation of the cervix, incision of the levator ani. These incisions may heal rapidly, but sometimes they do not. Rapid delivery, to my mind, even in so-called normal cases, presents difficulties that should not be passed over lightly. They are real. Furthermore, how about the patient later? Can we safely divulse the cervix and then pull the baby through the canal without sacrificing the integrity of the broad ligaments and the sacro-uterine ligaments which hold the uterus in place, and after involution has occurred go back to their normal degree of tonicity and keep the uterus there and prevent prolapse? I believe that the idea that prolapse of the uterus is caused by the stretching of the sacro-uterine ligaments and broad ligaments is correct, and if we are going to stretch these ligaments before the uterus is retracted over the child, and pull them and displace them downwards, to the degree necessary to draw a full-term child through an imperfectly dilated cervix, we lay the foundation for a large number of cases of prolapse of the uterus in later life. I think I have said sufficient to indicate the dangers attendant upon rapid forced delivery. I shall not go into all of the points I have written down, but will endeavor to bring out the more important ones.

I agree with Dr. Newell as to the futility of determining whether or not the head will go through a given pelvis by means of the application of forceps. There are too many factors concerned in the delivery of the head through a pelvis to allow one criterion to guide us in forming judgment. A head which at the beginning of labor presents the appearance that it will never go through the pelvis will often mold itself and pass through in a few hours of labor. A head that is grasped with forceps, and does not select favorable diameters, may later deflect, and present, e. g., a large parietal boss over the pubes. It may flex too much on the pelvis, and when nature would have shifted the head to the side and put the head in a degree between flexion and deflection to procure engagement, the forceps disturb the mechanism.

The other method, the Müller method, is likewise unreliable, for the same reasons. Again, forceps will not show whether the head is hard and unmoldable, or relatively soft and moldable, although they may, to a certain degree.

Whether to wait until after full term to induce labor, or whether to select a time when the woman shall be at full term to induce labor, or whether to wait until she shall have pains, is a subject on which I have some opinions. I do not believe in allowing a woman to go over term, for with the estimation of the time of pregnancy from a given date, and frequent measurements of the head in the later months of pregnancy with relation to the pelvis, we can determine when a woman has a viable child that will go through the pelvis, and under these circumstances I would not allow her to go over the probable end of her pregnancy. I am confident that we lose a large number of children by overgrowth of the child, by what was called by the old authors "overripening of the egg," and by certain mechanical difficulties which arise in labor consequent on hardening of the baby's head. In so far I agree with the authors that we should interfere at the end of pregnancy by slower, not by rapid methods.

There is one point I want to emphasize, and that is broadening the indications for Cæsarean section in behalf of the child. not so much to save the nervous strength of the mother as in the interests of the baby. I am acquiring an abhorrence of the spectacular methods in delivering some women which it is the lot of some obstetricians to do in cases of contracted pelvis of moderate degree. To do version, followed by powerful extraction, with Smellie-Veit from below, and dragging the head forcibly through a narrow pelvis, subjects the infant's soft head to hard pressure, which results in fractures, or very often in death of the baby, and, what is more important, injury to its brain. High forceps operations are particularly fatal to the baby and to its health; and therefore, if I ever followed in the footsteps of the essayist, I should prefer Cæsarean section to the other method of delivery from below, because even in a normal pelvis a high forceps delivery is not without risk. This view of the dangers of labor through a contracted pelvis is being noticed by neurologists. Sachs of New York has recently written a paper along this line, based upon statistical information.

DR. GEORGE SCHMAUCH: I must say that the essayist has referred to a class of cases I have not

yet had. I can remember only one death of a woman in the first twenty-four hours of the puerperium. This was a serious case of contracted pelvis. The woman was in labor for three days, had been frequently examined, and died possibly from sepsis and exhaustion. I regard labor as physiological and normal. The Doctor gives us some statistics about animals, and the length of time necessary to accomplish delivery, but he does not say a word as to the period of dilatation. Every one that has lived on a farm knows that frequently it takes a cow more than a few hours to give birth to a calf. I do not know whether veterinarians have made observations regarding the duration of the period of dilatation, but it is safe to say that we cannot expect dilatation in a woman in three or four hours. This frequently requires, in primiparæ, ten, twelve, or fourteen hours. Whenever I have delivered by high forceps a floating head, with incomplete dilatation of the os after a long period of pains, I must confess that I have made a mistake in the majority of cases. Certainly, when we see such a patient the next day, and she says she feels well, our first impression is that we deserve her thanks and praise, but later we come to an opposite conclusion. Doubtless it is our duty to ease labor as much as possible, and I agree with the essayist that the application of low forceps in multiparæ does not need any indications. Forceps should not be applied, however, before the os is completely dilated. I believe that prolonged labor is a sufficient indication for the use of high forceps, and it is my belief that such application is no more difficult than low forceps. Pubiotomy may be necessary in some cases of contracted pelves.

I remember a woman who had a large goiter, complicating pregnancy. She was nine months pregnant, and was hardly able to breathe when she came to the hospital. Tracheotomy, with subsequent partial thyroidectomy, was performed by a well-known surgeon, while we did a Cæsarean section. The woman died in three days, and the child in four. This is an example of surgery applied to obstetrics.

Not long ago I saw a woman who was delivered by a midwife after a physician had given her up and left the case. She had an immense goiter, and was a typical case of Basedow's disease. I saw her soon after labor. She was not able to lie down, was very cyanotic, had a pulse of 180, and she could not swallow liquids. There was an extensive œdema of the lower half of the body. The midwife had delivered her safely of a four-pound baby. That woman is well; also the child. Had I seen her earlier, I probably should have proposed vaginal Cæsarean section, but she escaped the benefits of modern surgery.

Toxæmia of pregnancy has been mentioned as indicating surgical delivery. I always object to the term "toxæmia," for, in my judgment, it means just as little as the mystic word "catarrh." The only difference I can see between them is that there is not yet a "Peruna" on the market for toxæmia. There is no condition in which we find such serious intoxication as in eclampsia, and yet eclamptic women have excellent labor pains. The child is frequently born before forceps can be gotten ready. I am slow to adopt Cæsarean section, unless there are strict indications. I would rather attempt forceps, version, or many other things, before subjecting a debilitated woman to an operation that requires prolonged anæsthesia. I would not do a Cæsarean section on one who is in an advanced stage of labor or who has been examined internally. There are many ways of treating inertia uteri, as, for instance, chloral, morphine, hot baths, hot vaginal douches, etc. There is no doubt that there is an inherited inertia which might compel us, in dealing with contracted pelvis, to resort to surgical means. There is also a difference in women's labor pains. Brunettes have better pains than blondes.

I understood the essayist to say that the morbidity after operations is less than that after normal labors. Statistics of German hospitals show the reverse. I should like to ask how these women get along in the puerperium after accouchment forcé. The cases which I have delivered before dilatation of the os always show bad scars.

DR. CHARLES S. BACON: I suppose that the reputation of Chicago has extended to Boston, and that the guests of this evening know that we are noted for our pugnacious propensities. We have a medical society in Chicago whose motto is, "If you see a head, hit it." That is not our motto, but we act upon it, and Dr. Reynolds will, of course, know and realize that we are treating him just as we would treat one of our own number. Early in the evening he felt very much at home here, and we are now showing him that we feel very much at home, in that we are treating him exactly as we would treat one of our own members. We also compliment him by assuming that he is abundantly able to take care of himself. He has shown that in his paper.

We have listened to an extremely interesting and scholarly presentation of this subject; and Dr. Reynolds is such an accomplished rhetorician that there is danger of his persuading us, perhaps against our judgment, so that we feel perfectly free in opposing some of the arguments. We shall consider,

then, the objections to rather than the good points of the paper, according to our habit.

Dr. Reynolds has proposed the operation of Cæsarean section in cases that have not been considered in the past as indicating the operation. We are not considering patients who are diseased, who have well-known pathological conditions, like diseases of the heart, kidneys, etc. We are speaking of cases of debility in pregnant women, and it is proposed to operate on them before labor, or at the beginning of labor, by surgical means. A great difficulty is this: We have not any units by which we can measure debility. We have no way of measuring debility, any more than pain or exhaustion. Any two men seeing a woman in labor after a certain time will have opinions that differ as to the amount of exhaustion, and until there is some way of measuring definitely exhaustion or debility, we shall have great difficulty in agreeing upon the method proposed. Then, of course, there is the question of determining beforehand the amount of obstruction there will be. That is also a matter that is known to be very difficult. Whether the uterus is weak or strong is a very difficult thing to determine. I have in mind a case that is an apt illustration. The patient was very weak during her entire pregnancy. She was in bed during most of the pregnancy. She had a very weak musculature, a weak physical condition. At the time labor was expected, there were a few pains, but labor did not occur. The pregnancy continued for a week or more. External examination showed an extremely thin uterus, so thin that the fœtal parts could be felt as if they were outside the uterus. The condition led even to the suspicion that possibly we had to do with an extrauterine pregnancy. After consultation, labor was induced by rupturing the membranes and thereby the introduction of the bag. Labor went on, after starting, in a very satisfactory manner. The contractions were good. Labor was terminated normally, without any trouble afterward. If there ever was a case in which I expected trouble from a weak uterus, that was the one, and still there was no trouble. I cite that as in illustration of how difficult it is to find a measure of debility.

The proof that a patient suffers from a labor, proof from the progress of the puerperium, is rather difficult to accept, as it is dependent upon so many other factors. One fact Dr. Newell brought up is quite worthy of more consideration. He referred to a few cases he had seen, where, shortly after labor, from half an hour to two or three hours the patient was in a condition of extreme exhaustion, as if from hemorrhage, but where there was absolutely no hemorrhage. I have seen cases of that kind myself, but I am quite positive, after a study of some cases of that kind, that that condition was not due to exhaustion. In the cases that I saw I noticed that we had to do with very severe continuous contractions of the uterus, constant after-pains, which led to a state of profound shock. We all know how much trouble after-pains may occasionally give. These are simply extreme cases. In the first case I had, the symptoms yielded to large doses of morphine, and I am convinced, from the description given by Dr. Newell, his were cases of that nature.

I would suggest, that if we are to reach some agreement on this subject, we shall have to arrive at some way of measuring the debility or condition of the patient, and we must try to include those cases with those where there is some definite pathological condition. I think we agree that in cases of heart disease, kidney disease, and other conditions, operative delivery before labor begins is sometimes right. Can we not, in the cases Dr. Reynolds has described, by a more thorough and systematic examination of the blood, examination of the condition of the circulation, by proper measurements, determine the pathological conditions that will give us some basis of agreement? If so, they can be put on some plan, and we can attain a uniform practice.

Dr. Rudolph W. Holmes: When Dr. Reynolds read his paper before the American Gynecological Society, at the meeting held in Chicago some years ago, advising the tentative application of forceps in minor grades of pelvic contraction, and then, in the absence of descent, to perform Cæsarean section, I questioned the expediency of such a course, and I still am of the opinion I held at that time. I believe the danger of infection is enormously increased by the introduction of the blades of the forceps within the uterine cavity, even in the hands of the most expert and careful obstetrical surgeon. I believe the risks attendant upon the high forceps operation in the presence of even minor grades of pelvic contraction vie with or even exceed the dangers of a timely primary Cæsarean section in the hands of an expert operator. Naturally, the risks in high forceps-operation are directly in proportion to the degree of the cephalopelvic disproportion. These statements being true, as I believe, it would seem more nearly scientifically correct to perform section, even if by so doing the thought remains that the child might have been successfully delivered by high forceps operation. In making these remarks I would not be understood as advocating the general or promiscuous use of Cæsarean section.

Every obstetrician has had cases of the class

mentioned by Dr. Reynolds. I have had some which properly would be classed in his category, but I have been unable to formulate any plan of procedure for their treatment; we all have encountered patients who have aroused our gravest forebodings of danger, and yet who easily and safely passed through the labor; on the other hand, women who promised the most auspicious labors have done exceedingly badly. To cull these unfavorable cases and then treat them according to Dr. Reynolds's suggestion will tax the diagnostic acumen of the most expert clinician. My plan generally is to allow the woman to go on to labor, and then to act according to circumstances. This is merely the orthodox procedure now in vogue. I hold myself a disciple of Schauta and Olshausen, who maintain that the proper time for Cæsarean section has gone by when attemps have been made by forceps or dilatation, or even vaginal examinations made in immediate relation as regards time to the operation; yet I wish to outline briefly a case in which I did a Cæsarean section after attempting to dilate a rigid os. This woman was one in the category depicted by Dr. Reynolds.

A girl, illegitimately pregnant, was admitted to the Cook County Hospital on the 29th of October, with a pulse of 140. She gave a history of repeated operations for osteomyelitis of the right leg. In the later months of pregnancy she has had some dyspnœa. She went on fairly comfortably until November 17th, when she had irregular contractions, which lasted a few hours. On the 6th, 7th, and 8th of December, contractions recurred intermittently. On the 9th, severe attacks of dyspnœa developed. Emesis appeared on the 8th, and persisted. By the next day the emesis was very severe, requiring rectal alimentation. On the 10th, the dyspnœa was very distressing, and the pulse became irregular. In addition to the myocarditis, it was now determined that she was also suffering from toxæmia of pregnancy, a term which Dr. Schmauch derides. The urine at no time showed abnormal constituents, although it had become progressively more scant. At this time it was determined to terminate the labor. Under ether anæsthesia, attempts were made to dilate the elongated cervix. Before the finger could be introduced, the cervix began to tear, so further attempts were not made. A strip of iodoform-gauze was placed in the cervix, and arrangements made at once to transfer the patient to the surgical operating-rooms. During this interval, Dr. Frederick Tice saw the woman, and corroborated the medical diagnosis, and suggested that the woman should be delivered in the quickest way possible, in order that the baby might be saved, and stated that he believed the woman was in such a precarious condition that she would hardly survive the operation. As Dr. Tice's opinion coincided with mine, she was quickly prepared, and the section was done with the assistance of Drs. Krost, Vaughan, and Harger of the house staff. A living male child was delivered. By rare good fortune, the woman recovered, although on the second day the temperature was 101.4°, and thereafter it was essentially normal. The pulse for days post-partum oscillated between 90 and 130. I may say that there was another contributory factor in this operation. The patient had a pelvis slightly below normal, a vera of 9.5, which, however, would have aroused little or no concern if she had been a normal woman. A vaginal Cæsarean section was not done, as I believed an abdominal operation could be performed more quickly.

I must agree with Dr. De Lee, in that I believe manual dilatation and high forceps are too portentous operations to be lightly undertaken. Their combined dangers are no less than a primary Cæsarean section when done under the proper circumstances and surroundings.

Dr. Harris, who so easily and quickly dilates the uterine os manually, has recently invented an obstetrical dilator, which suggests he must have at last found a cervix too resistant for manual dilatation.

Dr. Frank Cary: I have been much interested in Dr. Reynolds's paper, and desire to compliment him on his courage in presenting the ideas which he has advanced here. What strikes me in the paper is that the essayist gave us a general picture, in which I concur. He has not given me any points, however, which will help me out in coming to a conclusion upon which cases I should operate. Apparently, when we have a picture of a severe case, the patient will go through labor without great difficulty, and labor may be terminated quickly if the measurements are correct. It is often said that we have more difficulty in delivering patients in the higher than in the lower walks of life. I have had the greatest difficulty in delivering some women in the lower walks of life. It seems to me, it will be a long time before the obstetrician can attain such a state of perfection in judgment that he will be able to say just which case should and which case should not be operated on in a Cæsarean manner, where only the neurasthenic type of patients is considered.

I want to refer to another point made by the essayist. I am a little bit of an agriculturist myself. He said that cattle have little or no difficulty in delivery, and that the process goes on normally. So far as my experience goes, cattle often have a great deal of difficulty. Veterinarians make much

money by delivering cattle. Retained placenta is a common condition in cattle, and not infrequently it was common for them to go through the country, "unbuttoning the placenta," as they call it.

When I can have some means by which to determine the size of the child's head before the woman goes into labor, then I shall be very glad to accept the position taken by the essayist. But I find it exceedingly difficult to tell the size of the child's head before the child is delivered, and I know that a great many heads that are delivered with forceps, after I have given them time to mold, go through without great difficulty. It strikes me that I should often have considerable difficulty if I were to apply forceps to the head and undertake to "draw it in" before it had molded.

DR. J. CLARENCE WEBSTER: Dr. Reynolds began his paper by referring to the evolution of the female pelvis, and I think he did well to emphasize the point he brought out; namely, that in the transition from the lower mammals to the human female, there is an alteration in the bony pelvis, the canal, from being straight, becoming curved, and the relations between the diameters at the inlet and outlet being altered. There is also the development of the promontory of the sacrum and the sacral curve, as well as other changes which he did not mention. We know that in the evolution from the higher monkeys to the human animal there is a marked change as regards the soft tissues of the pelvis. It has been clearly demonstrated that the pelvic fasciæ play an unimportant part in the composition of the pelvic floor in animals lower than the human female. Even in the chimpanzee, the ourang-utang, and the gorilla, they are very little developed. The pelvic floor muscles are, however, better developed in the latter.

Dr. Reynolds did not refer to another very important factor; namely, the psychical. The psychical element does not play a part, so far as we know, in the mammalians below the human female, and it must be regarded as exercising an important influence, in the latter, not only in pregnancy, but in labor and the puerperium. Moreover, we must admit that marked differences exist between females of the human race. The lowest races approximate nearer to the mammalian type. The highest civilized races vary most from it; and the more artificial the state of civilization, undoubtedly the greater the percentage of abnormalities in labor. Dr. Cary's statements with regard to the cow are perfectly correct. I understand that they hold true with regard to other animals; that the more highly bred animals are, the more domesticated they are, the more complicated is labor, and the complications are mostly of a physical character, but possibly there is a nervous element to be considered.

Now, having made those statements, Dr. Reynolds says that labor in the human animals is not the natural, simple process that it is in the mammalians; that in the evolution of the high qualities of the human race there has not been a development *pari passu* sufficient to make labor an easy, natural process.

Dr. Reynolds admits that there are conditions in the bony pelvis, as obstructions from tumors, etc., which necessitate interference. He also admits, that there are certain defects in the soft tissues and certain diseased conditions which necessitate artificial delivery. But he brings up a class of cases not generally regarded as affording indications for artificial delivery. This is the point of contention, and I think his contention has not been kept clearly before those who have spoken to-night. The question for each of us to consider is, Is he correct? Have we met with such cases? Is interference justifiable? For my own part, I am not at all in a position to make any positive statement; but I feel that there is one thing that is too much lost sight of by the general public and by the profession; namely, that labor in the human subject is not the simple, natural process that it is so generally stated to be. In our treatment of obstetrical cases we may be too prone to act as if it were. It is refreshing that a man should come from the East, from a conservative old city like Boston, urging such bold views. It is not quite as radical a procedure as that of which I have recently heard, employed by a very distinguished and well-known New York obstetrician, who makes a routine practice, in all ordinary cases of labor, as soon as full term is reached, of introducing a bag to dilate the cervix, and later delivering with forceps. (Laughter.) That is more advanced than the Boston procedure. It has not yet been brought before our Society, but I would make the suggestion to the committee that the practitioner be invited to read a paper before the Society at some future time.

With regard to Dr. Reynolds's paper, one point is of great importance. It is intended only for obstetrical experts, at best. The general adoption of his suggestions by the profession would probably lead to great abuse and to increased risks in childbirth.

DR. JUNIUS C. HOAG: I wish to say that I am essentially in accord with the writer of this thesis; but I do not believe in introducing the hands or the forceps into the uterus, if a Cæsarean section is probable, neither do I believe in mixing the indications for high forceps and version. I think it is

useful to have general rules by which to be guided, and have long been guided by the rule that where the indications for forceps begin, there the indications for version cease. That is to say, we should not, in a doubtful case, apply forceps and then abandon the procedure and do a version, as a general rule; neither should we interfere too far in the uterine cavity, and then finally resort to Cæsarean section. But, undoubtedly, there is a class of debilitated women who, as Dr. Webster well remarked, can better be delivered by some operative procedure; but a distinction should be made between poorly developed women and those who have pelvic deformities. The essayist seems to restrict his cases to the first category, and there can be no doubt that there is a certain number of debilitated women who would be far better off if they underwent Cæsarean section. I have not the slightest doubt of that. But the question remains, as several of the gentlemen have mentioned, Who is to be the judge, and where is the line to be drawn? It can only be drawn in the light of experience. It is impossible to teach a medical student obstetrics so well that he can go out and tell where the line is to be drawn. It is hardly possible for those of wide experience to determine infallibly what to do, but that there is a class of cases in which it may be determined, there can be no doubt whatever, and I wish to refer briefly to a case which I think I reported before this Society, which will show my own views in the matter.

I had a patient in the hospital, who had been having fever for a long time. She had lost in weight, and was a poorly developed subject to begin with, and finally she weighed no more than seventy-six pounds. Her time for labor had come, but labor pains did not come on, and I felt that Cæsarean section would be the easiest thing for her. I did the operation, delivered her of a large child, say seven or eight pounds, and on the fifth day she wanted to get up. On the seventh day, as I remember, she did get up, and left the hospital at the end of two weeks.

In a case like that there can be no question as to the choice of operation. This was an extreme case.

Dr. Frank S. Churchill (by invitation): May I say a few words from the point of view of benefit to the baby and also of the type of women referred to? Dr. De Lee spoke of the benefit to the baby and of the advantages which would accrue to the mother from a Cæsarean section, rather than to subject a woman to a prolonged labor or to the danger of forceps delivery. I believe that there would be fewer paralyses, cerebral, peripheral, and spinal, by following the method advocated by Dr. Reynolds, than by either the application of forceps or a very much prolonged labor.

Another part of the paper interested me very much, which I should like to speak of briefly, as it is in the direction of that of which I have thought considerably since my residence in the West. It was my lot to be brought up among the men and women to whom Dr. Reynolds referred, and I know intimately the class and type of which he speaks. There are fewer of that neurotic type in this part of the country than there are in the East. There are fewer neurasthenic young girls in this part of the country, fewer cases of nervous prostration, than there are in New England. I cannot help thinking that that has a good deal to do with our attitude here in Chicago, in contrast to the attitude Dr. Reynolds takes with regard to those cases. I should like to make this prophecy, however, that in the course of time we shall have just as many of these same neurasthenic girls and women in this part of the country as they have now in the East, unless certain dangerous tendencies in our national life and education are checked. Every fall I am consulted by mothers as to whether they should allow their children, especially the girls, to go to school five days in the week and be allowed to carry out the complete school schedule that is mapped out for them. The mothers fear it is too much for their daughters. And every spring I see an increasing number of cases of chorea that are the result of the high pressure to which these young girls are subjected during the winter. I think it is reasonable, therefore, to expect that in the future there will be an increased number of these neurasthenic cases, and that we shall see more of that type of women described this evening than we do at present, and that perhaps then we shall agree with Dr. Reynolds in the method he has advocated to-night.

It will be of interest if, in the future, obstetrical surgeons will, in reporting their cases of Cæsarean section, note the nationality, to see what if any part nationality plays in the development of these neurasthenics.

Dr. Frank W. Lynch: We should not, in our discussion, lose sight of the fact that the paper of the evening presents a view-point which is new to most of us, although it probably is but an old story to the essayist. Moreover, we must keep in mind that Dr. Reynolds has stated that, as a result of his study of this class of cases, he believes he may, as a rule, prognosticate the course of labor. These points are of the greatest importance in our consideration of this paper, because we all individually believe that we are better able to discuss our own cases properly than other men, and we furthermore

believe that we, as specialists, have adopted our methods of treatment as a result of the tabulation and study of our past experiences.

As for myself, the ideas that have been advanced by the essayist are new, and I do not feel competent to discuss them, for the reason that I have not yet tabulated and studied my cases from the standpoint advanced this evening, and partly for the reason, which I believe holds true with others who have participated in this discussion, that the bulk of my experience, as theirs, has been obtained in hospitals in which the patients have been drawn largely from the lower walks of life, in whom the nervous element is not paramount. Consequently, morbidity from this cause has not been a prominent feature in the puerperium. If it develop later, we do not see it, for the patients have left the hospital, and we do not follow all obstetrical cases as we do surgical, to study the late results. We all admit, however, that there are cases in which the morbidity from exhaustion may be considerable. Doubtless we all could cite many instances of such, which, in our critical review, we have classed as badly treated.

If, then, by study along these lines we may come to the point at which we may dare to prognosticate the labor of an individual case and its effect, we also may dare to resort to primary delivery before the case has dragged on to a point at which active interference is demanded by the unfavorable condition of the patient. Personally, I believe that the high forceps operation is a serious one, and that there should be an indication more marked than inertia to warrant its use. In this I seem to differ from some who have participated in the discussion.

I am sure we have all received a stimulus from Dr. Reynolds's paper, which will not fail to turn our interest to the study of this class of patients.

Dr. Effa V. Davis: I would call the attention of the essayist to the fact that while he speaks of taking good care of his pregnant patient, I believe the care usually given to these women is not of the proper kind. He reports two patients upon whom Cæsarean section was performed, their babies weighing nine and ten pounds each, but emphasizes the fact that they were delicate women. It occurs to me that if they were able to produce such babies they might have been made to increase their own muscular strength and endurance by a proper direction of nature's forces. The Doctor has given us the method by which athletes are trained. Every woman who expects to become a mother has rather a strenuous piece of labor to perform, and instead of training them for this athletic business, they are usually put to bed, kept very quiet, and hardly allowed to step out. This has been called good care. In my judgment, even delicate women, such as those described by the essayist, might have a much better musculature and smaller babies if they were previously subjected to certain physical training. If we expect women to be mothers, they must be trained for mothers. Muscular exercise is too much overlooked, considering the trying muscular ordeal through which they have to pass.

I have come to believe this, through my connection with a semi-charitable hospital, where patients are brought to await confinement several months. We never permit them to sit idle, but oblige them to work at rather hard tasks, and find that it is beneficial. I published some conclusions which I had reached on these cases, two years ago, and while the number of cases was comparatively small, the figures show that the women who worked hard during pregnancy gave birth to only average sized babies, and their labors were easy. A nine or ten pound baby is too large for any woman to bring forth, whether she is a neurasthenic or not.

I fully agree with the essayist, that the class of cases which he emphasizes exists, and when they get to the point of delivery, it is a hard matter to decide what to do. A case of exhaustion came under my observation, which was alarming for some hours, the pulse went down to 40 or less, and remained so feeble that the patient had air-hunger and the appearance of having an internal hemorrhage, but in no way could her condition be explained except as exhaustion. This was the fourth birth, and the largest of all her babies (eight or nine pounds), which were born in rather rapid succession. She said she had taken the "best care" of herself she ever had, which meant that she had taken no exercise, and this was the only time she had experienced anything of the sort after her labors.

Dr. Rachelle S. Yarros: I want to add my thanks to the essayist for bringing this subject before this Society in the manner in which he has presented it. I was brought up, medically speaking, in Philadelphia, by excellent obstetricians. The idea of conservatism, which meant the strictest indications for any interference, was early instilled into me. In my subsequent work, all my associates at dispensaries, hospitals, and college have been gentlemen of the same careful and conservative sense, but yet my own experience has gradually forced me to the conviction that a very material change will have to be made in the line of indications for interference. It requires great courage to advocate a change in that direction, because most of the best men in this country and abroad are conservative. There is one point that I wish to emphasize especially, and that is, the danger

to the mother's general health, which frequently results from a prolonged, but what we call a perfectly normal, labor. Somebody in this audience said that this would apply only to the women of the well-to-do or what we call upper classes. I dissent from this view entirely. My work is mostly among the poor, and I have sufficient evidence to prove that there is practically no difference. I can recall a large number of cases where it required a number of months to regain good health after a normal labor, in women apparently perfectly well. I also know a number of women who have grown prematurely old and worn from no other cause than two or three prolonged labors. I am sure that many doctors who see a considerable number of obstetrical cases have had the same experience. It seems to me, therefore, that from the standpoint of morbidity to the mother, more liberal aid is distinctly indicated during prolonged first or second stages of labor.

DR. A. BELCHAM KEYES: I should like to make one remark. When a woman has been in labor for, say, three or four days, during which time she has had many vaginal examinations, infection may be far advanced before the labor terminates. I believe this early infection should be recognized as causing some of the cases of so-called uterine inertia.

DR. EDWARD REYNOLDS (closing the discussion): I have been much gratified with the discussion my paper elicited. I want to thank Dr. Bacon for his kindness in receiving me into the Chicago family, as I like nothing better than a man who says what he thinks, particularly if he differ from me, and I will take the same liberty in replying.

I noted a great many points during the discussion which I will not take time to mention. Dr. De Lee made one criticism which I should like to answer, to the effect that many patients will be able to simulate symptoms and deceive their physicians, or lead them to operate according to their fancy. The obstetrician whose patients can deceive him so easily as that is not the gentleman I am addressing. He does not belong in that class.

DR. DE LEE: That was a joke.

DR. REYNOLDS (resuming): With regard to what was said concerning the application of tentative forceps, I will revise my paper on that point. That is a misfortune or disadvantage, to some extent, in advancing the Cæsarean operation. Whenever pressure of the head into the brim is a satisfactory test, I would avoid tentative forceps, and the more experience I have, the less do I resort to that expedient. There are doubtful cases, however, in which that procedure is valuable, and when the obstetrician is aseptic and careful, as well as skillful, a very gentle use of tentative forceps is not a serious disadvantage, and is justified by the information it gives us.

I should like to say in regard to the President's criticism, of mixing up the indications for forceps and version, that it is legitimate. I was slightly misunderstood in regard to this in presenting my paper, and I quite agree with what he has said.

I was much interested in what Dr. Churchill said. It opens up a wide subject, one in which I have been interested for years. I was much interested in his impressions of people in different communities, but I believe you have in Chicago some of the class he mentioned.

The rest of the discussion is admirably summed up in the remarks made by Dr. Lynch. This is not a new subject. It is one that has been in my mind for many years. It is one that most men have not been working on. It cannot be settled *ex cathedra*. I wish to say to Drs. Bacon, Cary, and others, who have spoken on this subject, that I hope they will do as I have done. If they will take this point and keep it in their heads, and not use it until they cannot help themselves, I should like to wager a good dinner that they will agree with me in five years, and the only trouble is, that I should have to eat too many dinners at their expense. I ask you to remember this point and work it out, and do not use it until you cannot help yourselves. In that respect it is like getting married.

ABSTRACTS OF CURRENT LITERATURE

DEPARTMENT OF SURGERY

UNDER THE CHARGE OF

JOHN B. MURPHY, M. D. — E. WYLLYS ANDREWS, M. D.
FREDERIC A. BESLEY, M. D. — WM. R. CUBBINS, M. D.

X-RAY DIAGNOSIS OF PRIMARY CARCINOMA OF THE LUNG. By F. Otten.

Otten details his experience in twenty-one cases of primary lung carcinoma studied at autopsy. Thirteen of the cases were radiographed during life, and these interesting radiographs are reproduced in the article. The X-ray examinations show that the development of lung carcinomata varies greatly. Most often, the growth takes in a whole lobe; but with almost equal frequency the neoplasm starts as a small focus at the hilus, surrounded by similar foci in the immediate neighborhood. Finally, there is the type characterized by multiple foci of carcinoma scattered throughout the lung.

In establishing a diagnosis of primary lung carcinoma, the following conditions must be taken into consideration: The various other types of mediastinal tumor, aneurysm of the aorta, tuberculosis, gangrene, lung abscess, chronic fibrous pleurisy, and bronchiectatic cavities. An important differential point is, that carcinoma very seldom affects the upper lobe of the lung. According to Otten, a Röntgenograph renders invaluable aid in determining the site and extent of malignant infiltration of the lung. — *Fortschr. a. d. Gebiet d. Röntgenstrahlen*, bd. ix, h. 6. M. G. SEELIG.

CHRONIC ADHESIVE MEDIASTINO-PERICARDITIS AND ITS TREATMENT BY CARDIOLYSIS. By Dr. W. Danielsen. From the Surgical Clinic in Marburg, Professor Küttner, Director.

Danielsen reports a case operated upon by Professor Küttner. This patient had not been well for ten years, and showed all the typical signs of adhesive pericarditis; cyanosis of lips, inspiratory dilatation of cervical veins, and venous collapse during diastole. The heart was moderately enlarged, and did not move on changes of position of the patient. Systolic retraction of chest-wall was present. The pulse was small, irregular, and rapid, but pulsus paradoxus was not present. Apex beat not palpable. The liver was palpable and hard. Ascites present; 9.2 liters removed by trocar.

The operation was done by raising a musculocutaneous flap of horseshoe shape, its base being on the left anterior axillary line and its apex at the left sternal border. Subperiosteal resection of the fourth to sixth rib and costal cartilages was made, and the pleura carefully freed. Healing was by primary union.

Nine months later, the patient was found to be substantially improved, the following observations being made:

1. Heart action: Pulse full, regular, and strong.
2. Ascites entirely gone.
3. Subjective: Patient stated that he was well, and again able to work.

Danielsen defines mediostino-pericarditis as the presence of adhesions between the visceral and parietal leaves of the pericardium and the neighboring tissues as the result of a pericarditis. He divides the cases as follows:

1. Adhesion of the two leaves to each other — obliteratio or synechia pericardii.
2. Adhesion of the outer fibrous layer of the pericardium with neighboring structures, especially the mediastinal connective tissue, which is often synchronously inflamed. Pericarditis adhesiva externa, which, as it becomes more extensive, produces the pathologic complex known as mediastinopericarditis.

The cardiac muscle, also, as a rule, suffers, and secondary inflammatory and degenerative changes are often present. Endocardial processes of various types are apt to be found.

Danielsen favors grouping the symptoms in the following manner:

1. Anamnestic statements as to previous diseases and their remaining signs which are objectively demonstrable.
2. Obliteration of the pericardial sac with its complications:

(*a*) Mediastinal factors: Those symptoms which come on as a result of a pericarditis externa.

(*b*) Myocardial factors: Symptoms which depend upon the involvement of the heart musculature.

Danielsen found that, of nine cases, six had suffered from pleurisy of greater or less severity, one case gave a history of polyarthritis, one had had pneumonia, and one gave a negative history.

Clinically, the following points are brought out:

1. Absence of respiratory mobility of the lung over the heart.

2. Loss of normal variation in position of heart upon change in position of body.

3. Systolic retraction of the region of the heart apex. May be present when only slight adhesions are present, or even, sometimes, when none exist.

4. Friedreich's sign. Systolic retraction of the chest-wall, not only of the intercostal spaces, but also of the lower part of the sternum and the ribs. These retracted structures spring forward forcibly. This sign is held to be valuable.

5. Diastolic vein collapse (Friedreich), occurring synchronously with the springing forward of the structure referred to in 4.

6. Pulsus paradoxus (Kussmaul). This was found in only one of nine cases.

7. Irregular heart beat, depending on myocardial involvement.

Diagnosis. As a rule, not hard. In addition to above-noted points, the X-ray is, at times, of service. The following points have been observed:

1. Band-like filling of one or both diaphragmatic angles.

2. "Sac formation" in cardiac shadow contours.

3. A fluttering heart pulsation, not clear, on account of covering of heart borders by adhesions.

4. Diminished mobility of the diaphragm.

Treatment. Cardiolysis as devised by Brauer.

Indications. 1. When by conscientious use of medical means no improvement has followed.

2. Case with no chest-wall retraction should only be operated on when marked congestive symptoms, especially liver cirrhosis due to congestion, appear.

Technique. 1. Musculocutaneous horseshoe flap, its base at the anterior axillary line and its apex at the sternal border.

2. Subperiosteal resection of ribs, from third or fourth up to the sixth or seventh, extending from the sternum to the anterior axillary line. Resection of sternum, as a rule, not necessary.

3. Careful removal of the periosteum, to prevent new bony formation.

4. Careful hæmostasis. Suture.

5. Narcosis. General anæsthesia is recommended. Local anæsthesia has been tried unsuccessfully. — *Die Chronische adhäsive Mediastino Pericarditis und ihre Behandlung durch Cardialyse. Beiträge zur klinischen Chirurgie*, bd. li, 1906.

W. C. DANFORTH, M. D.

SURGICAL TREATMENT OF PERFORATING GASTRIC ULCER. By Robert J. Le Conte, M. D.

Dr. Le Conte reports three cases of perforating gastric ulcer, two of which were acute and one chronic.

In the acute ulcer, laparotomy was performed within a short time, and the rupture sewed up firmly in such a way as to cause no pyloric obstruction. Drainage was used both over the area of perforation and in the pelvis, with the exaggerated Fowler position.

Recovery was complete and uncomplicated in both cases, and there has been no recurrence of symptoms, although the stomach has had severe tests.

He considers the indications for gastro-enterostomy to be: 1. Multiple ulcers in stomach and duodenum. 2. Where suture may cause narrowing of the pylorus. 3. Where the wound cannot be closed directly without the aid of an omental patch. In these cases the strain on the patch, due to post-operative distention, might cause rupture.

The third case was where there had been a previous perforation with an ulcer that ruptured into the lung. This was followed two years later by another perforation, that could not be reached, on account of adhesions. Drainage was inserted, but the wound in the stomach did not close, and the patient died from exhaustion.— *Annals of Surgery*, Dec., 1906. W. R. CUBBINS.

ACUTE GENERAL PERITONITIS WITHOUT DEMONSTRABLE LESION. By Edward Martin, M. D.

Dr. Martin reports three fatal cases of peritonitis in which there were autopsies, without being able to show any demonstrable lesion.

The author therefore wonders if there is not reason to suppose that this condition may not be more frequent than is at present suspected. — *Annals of Surgery*, Dec., 1906. W. R. CUBBINS.

A CONTRIBUTION TO THE OPERATIVE TECHNIQUE OF ORCHIDOPEXY.

The testicle is drawn down through an opening in the most dependent portion of the scrotum and sutured into an incision in the thigh upon the same side. The sutures are passed through the tunica vaginalis and thence to the deep fascia of the thigh. The skin and the scrotum are then sutured carefully together and sealed up. The child's limbs are held together for several days; then he is allowed to walk. Later the testicle can be loosened and placed in the scrotum. De Beule reports two successful cases.—*Jour. de Chir. et An. de Soc. Belge de Chir.*, 1906, No. 8, p. 390. W. R. CUBBINS.

ABSTRACTS OF CURRENT LITERATURE

DEPARTMENT OF GYNECOLOGY

UNDER THE CHARGE OF

J. CLARENCE WEBSTER, M.D. — E. C. DUDLEY, M.D
JOHN C. HOLLISTER, M.D. — F. W. LYNCH, M.D.

ABSCESS OF THE UTERUS. By Salva Mercade.

The term is applied only to a purulent collection encysted within the uterine parenchyma. So defined, the condition is rare, because of the tendency of the whole organ to become involved (due to its vascularity). The fact that the condition was first mentioned by Portal in 1803 is noted. Otto von Fran collected some sixteen cases, and the author, after careful study of the literature, now reports a collection of forty-one cases.

Ætiology. Any age.

Infections. 1. Puerperal; 2. Gonococcic; 3. Traumatic; or 4. Tuberculous.

1. *Puerperal Infection.* The most frequent cause (22 out of 41), which can be explained by: (*a*) Receptivity of enlarged uterus; and (*b*) Unclean instruments (abortions, etc.).

2. *Gonococcic Infection.* Next most frequent. The great divergence of opinion among various authors is noted. This author says he does not doubt the existence of such abscesses, although they are rare. "It is certain that the gonococcus penetrates with difficulty among the muscular fibers of the uterine parenchyma, yet Madlener has definitely proved their presence there. Access here probably acquired by the condition of uterus during pregnancy or delivery." "A uterine abscess has never been noted in the course of a chronic metritis, yet there is no reason why it should not."

3. *Traumatic.* Very rare. It is pathological or surgical solution of continuity, which acts as an atrium for the infection.

4. *Tuberculous.* "Tuberculous infection may cause the formation of an abscess in the uterine parenchyma, but it is very rare." Five cases reported — three in secondary tuberculosis of the uterus and two in primary cases.

Pathogenesis. "Uterine infection is directly dependent upon a physiological or traumatic uterine solution of continuity. But it is possible to admit that inoculation of the parenchyma may occur without such lesion as is shown in tubercular infections which may act here as in intestinal tuberculosis; i. e., penetrate to the deeper layers without a break manifest in the epithelium. The uterine mucosa is the most vulnerable of all genital routes of male and female to the tubercular germs. Other experiments have also shown (de Guzzoni degli Ancarani) that the tubercular virus can not only pass the uterine mucosa, but when introduced into the vagina is able to give rise to a general tubercular infection without any localization in the genital organs.

1. Inoculation by the circulation is exceptional. A uterine abscess has never been noticed in the course of marked septicæmia. Tubercular abscess of the uterus alone seems to appertain to that pathogenic variety, but it is still with some reservation that this inoculation by the blood route is admitted, even in the case of secondary abscess.

2. The formation of an abscess (without particular stress upon the tubercular abscess) is still a subject for discussion in acute cases. Bernut and Championière's idea that it forms in the lymphatics is discussed. "Abscess of the uterus constitutes, by itself, a single morbid entity, a complication, it is true, of a uterine infection, but an isolated complication, and not peculiar to the lymphatics of the uterus. We are forced, then, to admit that this (the abscess formation) is due to infected emboli from the uterine mucosa."

3. *Point of Election of the Abscess.* It is characteristic that all or nearly all are in the proximity of the uterine horn — whether the abscess is puerperal or post-abortive, whether it is of streptococcus, gonococcus, or tubercular origin. This is explained as follows: (*a*) By predominance of lymphatics; (*b*) By frequency of embryonal rests. It is to this second reason that the author attributes the formation of the majority of the true abscesses of the uterus, because in this embryonal *débris* there is so often present cystic formation which acts as an admirable place for the development of an abscess.

Pathological Anatomy. There are characteristic distinctions between the different kinds of abscesses of the uterus: (*a*) Acute, characterized by rapid formation; (*b*) Chronic; can develop unsuspected; found at autopsy.

Place. Great many on a level with uterine horn; usually at the fundus of the organ; more rarely they lie in the lateral walls or anteriorly. They are almost always abscesses of the body (exceptions noted); can be subperitoneal (most frequent), intramuscular, or submucous. The subperitoneal are to be distinguished from the pelvic abscesses. The intramuscular abscess is the true abscess of the parenchyma. Submucous abscesses are the most rare, often go unnoticed; may open spontaneously and discharge as an abundant "leucorrhœa."

Number. Usually single, though multiple are reported. Such cases are discussed.

Volume. Very variable, extremely small (tuberculous) to one which "attained the size of a six months' pregnant uterus." The size seems to be somewhat dependent on location, whether intraparenchymatous or subperitoneal.

Form. Depends on environment. Usually round; at times "stretched out."

Contents. Usually thick, tenacious pus, the quantity of which varies from a few grams up; one case containing two liters is noted. It is usually creamy, yellowish, or greenish; no particular odor; generally sterile in tubercular cases, but may contain abundant gonococci, streptococci, or associated microbes.

Environment. Depends upon location of development. The findings in the different cases are reported.

Disturbances in the Environment. Uterus slightly altered; increases in size, partly from reactional hypertrophy; three times normal size in one case reported; evidence of degeneration is noted in various cases. Evidence of reaction in the peritoneum also described in some cases (adhesions). Five cases of general fatal peritonitis following opening of abscess into peritoneal cavity are noted. May open into uterine cavity with spontaneous healing. As concomitant inflammatory lesions are noted those of abscess of the ovary, salpingitis, broad ligament abscess.

Clinical Aspect. Varies, whether with acute or chronic.

Acute. Onset usually abrupt, if coming after puerperal infection. Symptoms of uterine infection are evident. If coming as a complication in acute metritis, symptoms like those of uterine tumor develop. O ten very difficult to make exact diagnosis.

Leucorrhœa (may be purulent), altered menstruation, interference with bladder and rectum, pain later — paroxysmal, as a rule — or following examination, local tenderness, are all symptoms. Examination is not always definite. If no adhesions and the abscess is truly uterine, that organ is movable and enlarged. If in the neck, a fluctuating mass can be often distinguished, "but a collection elsewhere would speak as well for a purulent suppuration." If adhesions have already formed, one can only make a diagnosis of pelvic abscess.

General symptoms are marked when abscess is in parenchyma (chills and fever usually).

The affection is usually progressive until rupture into the uterine cavity or peritoneum; at times, progress is intermittent. Also, breaking into bladder and rectum have been noted.

Chronic Abscess. Much more rare; onset insidious; usually appears in the course of a general or local tuberculous infection. Acute symptoms may occur and then disappear, and may recur. The whole course may be latent and found only at operation or autopsy.

Prognosis. Of the acute, depends upon whether and when the abscess ruptures. Of the tuberculous, depends more upon generalization, etc.

Diagnosis. Extremely difficult.

1. At the onset it must be differentiated from puerperal infection. Here the progress of the infection is much more rapid, and symptoms stay more general.

2. "When the abscess begins, when there are no adhesions, and the location is in one of the surfaces or at the fundus, there is more or less fluctuation, and moves with the uterus. When fever and the general symptoms are found, then one can possibly make a definite diagnosis of the intrauterine abscess. Errors are frequent.

Treatment. Subordinate to anatomical location, cause, and complications.

(*a*) Abscess in neck (rare), simple vaginal incision and curettage.

(*b*) If bulges into cavity, dilate neck and open abscess.

(*c*) Laparotomy. Always, when abscess is in fundus (vaginal route not advised). If abscess is well isolated and organ otherwise normal and woman is young, puncture, incision, curetting, and packs with iodoform-gauze is advised. Also, cautery and pelvic drains of tubing and gauze are spoken of. When multiple abscesses and involved adnexæ, vaginal or abdominal hysterectomy have been done. Opening curettage and drainage of abscess cavity has given best results. Should not remove whole organ, if possible to save. Enucleation of abscess with surrounding musculature is to be recommended, so that primary union can be obtained. Usually necessary to remove adnexa of same side with horn. In all other cases, total abdominal hysterectomy is advised. — *Les Abces de L'uterus. Ann. de Gyn. et D'obst.*, Jan., 1907.

BOOK REVIEWS

CONSERVATIVE GYNECOLOGY AND ELECTROTHERAPEUTICS. By G. Betton Massey, M. D. Fifth revised edition. Philadelphia: F. A. Davis Co.

When we gaze upon the bleak background of our former experience in the use of electricity in medicine, we wonder that its advancement has been so rapid as to warrant the production of this excellent work of over four hundred pages, covering but a single branch of the subject.

While we differ from the author in some points of technique, and may also take exceptions to his position on some physical points, especially induction currents, yet that is only to be expected. Difference of opinion will arise, and is always helpful, otherwise we should never come to correct conclusions.

The fact that the fourth edition of this work was exhausted within the past year is not only a testimonial to the ability of the author, but also points significantly to the fact that the trend of the practice of medicine is toward more physical measures.

The illustrations are very fine, and, taken as a whole, the work is excellent, and should be in the hands of every practicioner who would use electricity intelligently in gynecological work.

C. S. NEISWANGER.

STUDIES IN GYNECOLOGY. Presented in the form of illustrated problems for classroom work. By John A. Sampson, A. B., M. D., Clinical Professor of Gynecology in the Albany Medical College; Gynecologist to Albany Hospital, Albany, New York; Fellow of the American Gynecological Society. Albany, New York: Fort Orange Press, Brandow Printing Company. 1907.

The subject of gynecology has been presented in this book in the form of studies or problems which are to be worked out by the students. The problems are stated and elucidated by questions which indicate the various features to be determined in the different phases of each subject, and then facts are portrayed by illustrations, drawings, explanatory legends, and accompanied by questions. Space is left for questions and answers.

It is intended that the problems to be discussed each day shall be studied previously by the members of the class, who are encouraged to consult text-books, periodicals, etc., after having thoroughly digested the problems. The space left after the questions is for answers which have been determined after classroom discussion, and for other notes which the student may subsequently wish to add.

The subject-matter has been so arranged that each advance is begun and finished independently of that which has preceded. By following this method, the student not only receives the benefit of his own efforts, but he has a practical book for future reference, and one to which he may at any time add new material, and in this way keep it up to date.

The idea, gained from classroom work, is a very clever one and well carried out, and the book presents a remarkably attractive appearance.

M. L. B.

THE AMERICAN ILLUSTRATED MEDICAL DICTIONARY. A new and complete dictionary of the terms used in medicine, surgery, dentistry, pharmacy, chemistry, and kindred branches; with their pronunciation, derivation, and definition. Over one hundred new tables. By W. A. Newman Dorland, M. D. Fourth revised edition. Octavo, 836 pp.; 293 illustrations, 119 in colors. Philadelphia and London: W. B. Saunders Company. 1906. Flexible morocco, $4.50, net; thumb-indexed, $5.00, net.

This fourth edition is certainly a delight to the hand and a pleasure to the eye. The beautiful soft binding of flexible morocco is an extremely pleasant change from the ordinary dictionary binding.

Special attention has been given to the definitions, with the purpose of making them clear, concise, and yet sufficiently complete, while, under the more important headings, a considerable amount of collateral descriptive matter has been included.

Besides the ordinary dictionary matter, a large amount of information has been included, arranged in tabular form. In addition to the usual anatomical and clinical tables, there are tables of tests, stains, staining methods, etc.

The name of its author, Dr. W. A. Newman Dorland, is a sufficient guaranty of the accuracy and comprehensiveness of the work. By the use of a large page, with a compact but clear typography, a large amount of matter has been put into a volume of handy size, and the different words, explanations, and definitions are arranged in a way that renders them easily comprehended with a minimum of effort.

M. L. B.

CORRESPONDENCE

THE SO-CALLED INCREASE OF UTERINE CANCER MORTALITY

SURGERY, GYNECOLOGY AND OBSTETRICS.

In the November, 1906, number of your esteemed journal there is printed an interesting article by Dr. P. B. Bland, entitled "A Statistical Study of Deaths from Cancer in the City of Philadelphia." As I have formerly dealt with this matter, I am perhaps justified in making a few remarks in connection therewith.

Dr. Bland writes that "uterine carcinoma is decidedly on the increase, as shown in accompanying chart. (Chart II.)." I should like to inquire if the author is thus so well convinced of the truth of his statement.

In Chart II we see that during 1879 there were 86 deaths from uterine cancer, whereas during 1904 there were 190. But, as appears from the chart of Dr. Bland himself, one cannot rely on figures based on statistics for one year only, especially when they are not very high, for there is sometimes much diversity. Therefore let us consider the first and last five-year periods. The mortality is, then, $478 \div 5 = 95.6$ during the former, and $815 \div 5 = 163$ during the latter.

In the mean while, what was the increase in population? In 1879 the total population of the city was 846,980, and in 1904 it was 1,408,000. Uterine cancer mortality statistics should have been based on these figures for the period 1900–04. Thus —

$$\frac{x}{95.6} = \frac{1{,}408{,}000}{846{,}980} = 158.9.$$

Therefore 158.9, instead of 163; a very slight increase indeed — two per cent or so.

However, before admitting that there really was an increase, we should study cancer mortality in relation, not to the total city population, but to the female population above thirty-five or forty, as cancer cases almost exclusively occur in this group. In such instance, who knows if during the last twenty-six years the adult female population did not show a relative increase in proportion to the whole? For aught I know, "one-child sterility" is an evil spread over both Atlantic shores. The term, at least, should testify to its existence.

In another respect, it seems that Dr. Bland did not heed the cases that died in town, that came from the suburbs or the country. Cancer patients willingly resort to towns, where hospitals, institutes, funds, and clinics of every kind, as well as practitioners of repute, exert an undeniable attraction. As an illustration of this I would cite Cracow (Galicia),[1] a university town, where, out of 1,744 fatal cases of cancer, 697 did not belong to the city population.

Then, respecting colored "ladies," we meet with the same disregard of the statistical status of Philadelphia. I make no pretence to a better knowledge of America; but, let me refer, at least, to the Reports of the Health Officer of the District of Columbia, where it is found that the difference between white and colored above forty years of age is only some 46 to 41. These statistics allowed even the writer to see that in females of both races uterine cancer mortality is nearly the same. Notwithstanding this fact, I am inclined to the opinion that a slight difference exists in favor of the black.

Finally, Dr. Bland is surprised at the frequency of cancer among foreign-born as compared with American-born. But one does not go to America in a cradle! The foreign-born population is, for the most part, composed of adults.

Consequently I am more than ever convinced that external and especially uterine cancer has not of late made any increase. Readers of SURGERY, GYNECOLOGY AND OBSTETRICS are referred, for further particulars on this subject, to the writer's work in the *Semaine médicale*.[2] Time passes quickly, and newspaper leaves fade as fast as those of trees. Nevertheless, my former contribution may still possess some value, inasmuch as a large amount of my *data* was drawn from valuable statistics from the United States.

I shall feel glad if these few remarks contribute to fortify the public mind against unjustified fear. I am well aware that statistics do not enjoy a very fair repute. Prince Talleyrand termed statistics *mensonge en chiffre* (a figure-lie), and the Germans are still more rude, in calling them *feile Dirnen*. But, although they may be deceiving at first glance, they are much better than the evil name fastened upon them.

R. DE BOVIS,
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[1] Statystyka miasta Krakowa, 1891–92, vol. iv.

[2] R. de Bovis. L'augmentation de fréquence du cancer, sa prédominance dans les villes et sa prédilection pour le sexe féminin sont-elles réelles ou apparentes? Semaine médicale, 10 septembre, 1902. Du rôle des principaux facteurs accessoires dans l'étiologie du cancer, Semaine médicale, 24 septembre, 1902.

Fig. showing anastomosis between spinal accessory and facial nerve.
a. End-to-end anastomosis between facial and spinal accessory.
b. Hypoglossal nerve.
c. Stylohyoideus muscle.
d. Parotid gland.
e. Central end of facial nerve.
f. Periphral end of spinal accessory.
g. Posterior belly of digastric muscle.
h. Muscle.
i. Outline of transverse process.
(Original drawing from a dissection).